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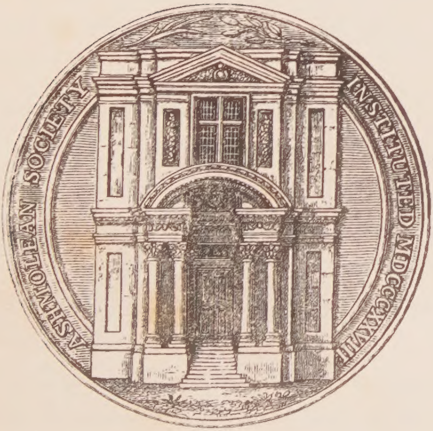

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ON THE

NATURAL HISTORY

OF

TWO SPECIES OF WASPS.

BY

THE REV. EDWARD BIGGE, M.A.

FELLOW OF MERTON COLLEGE.

OXFORD,

PRINTED BY S. COLLINGWOOD, PRINTER TO THE UNIVERSITY, FOR

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ON

THE NATURAL HISTORY OF TWO SPECIES OF WASPS.

READ TO THE ASHMOLEAN SOCIETY, FEB. 27, 1835.

THE following remarks upon the natural history of two species of wasps, both of which are common in Great Britain, are offered to the notice of the Ashmolean Society, in the hope that they may tend to reconcile the contradictions observable in the works of all the authors who have written on the subject, from the times of Swammerdam and Ray, to the present period. Very few original observations appear to have been made since Reaumur and De Geer turned their attention to the habits of these insects; and the accounts of later writers seem nearly all taken, and frequently without acknowledgment, from the works of those illustrious naturalists. It may indeed seem presumptuous in me to differ from such men; but it is perhaps better to state the points of disagreement at once, in order that the results of my own observations, made during the summers of 1833 and 1834, may furnish additional data for any members of the Society who may feel inclined to follow up the subject, and thus either verify or refute them.

The two species which are the subject of this paper, are the Vespa Vulgaris, or ground wasp, and the Vespa Britannica, or tree wasp. The former is common in all parts of the island; the latter, though occasionally met with in the southern counties of England, seems principally to abound in the northern districts, and in Scotland. It is by no means peculiar to Britain, as the name would seem to imply, but is spread over all the northern parts of Europe in great abundance. Some confusion has arisen with regard to the Vespa Vulgaris, whose habits are so ably described by Reaumur, in the sixth volume of his History of Insects. The species named Vespa Vulgaris by Linnæus, is in fact the Vespa Britannica, and is described by him, and by De Geer, as fixing its nest to beams in sheds, or under the eaves of houses, or to low trees; but the French entomologists afterwards bestowed the specific name Vulgaris upon that wasp which was most abundant in their own country, and in the southern parts of Europe, and whose nest is always formed in the ground. The error seems to have hitherto passed unnoticed, except that Shaw remarks that some confusion seems to prevail on the subject. The species now called Vespa Vulgaris, seems to be the Vespa Gallica of Linnæus; but as he does not mention the situation of the nest, it is not quite certain. He merely says, " Habitat in Europa Australiori[a]." Fabricius describes it as "Vespa Gallica, affinis Vulgari, at minor[b]." Mr. Wood, in his "Linnæan Genera of Insects," has figured the Vespa Britannica under the name of Vespa Vulgaris of Linnæus, and so far he is correct. But he describes the species as living in the ground, which is not the case with the Vespa Vulgaris of Linnæus. Leach seems to have been the first who gave the specific name Vespa Britannica to the tree wasp; but its habits and economy had already been fully described by De Geer[c].

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[a] Linn. Syst. Nat. vol. ii. p. 949.

[b] Fabricius, Genera Insectorum, vol. i. p. 460, ed. 1781.

The points of difference between the two species are as follow:

1. The tree wasp (Vespa Britannica) has a reddish brown spot near the point of insertion of the wings. It is seldom visible in dried specimens.

2. In the males and neuters, the base of the antennæ is yellow on the outer side, instead of being entirely black, as in the ground wasp; but the females often present exceptions to this distinction.

3. The tree wasp has two yellow spots on the back part of the corselet, whilst the ground wasp has from four to six.

4. The spots on the abdomen of the tree wasp are not so much detached from the black bands as in the other species, and, I think, less so in the males than the females. The markings on the abdomen, however, vary so much in different individuals that they cannot be safely relied on as a criterion of species. Linnæus drew a distinction between the hornets (Vespa Crabro) and the true wasps, founded on these marks, but it certainly cannot be considered as decisive; and the varieties enumerated by different authors, and particularly by Geoffroy, shew that the attention paid to this point has tended to create confusion rather than lessen the difficulty. Wasps taken from the same nest often present considerable variations in the marks on the abdomen, and no judgment can be formed from specimens in cabinets, which undergo great changes in drying.

5. Independently of the markings just mentioned,

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[c] Vol. ii. p. 766 et seq. pl. 26.

the tree wasp has more black upon the body generally, than the other species.

6. The tree wasp is rather larger.

7. The organs of generation in the males of the two species, as shewn by the plates in Reaumur and De Geer, vary considerably.

8. The abdomen in each species contains the same number of rings, viz. six in the females and neuters, and seven in the males.

As the object of this paper is not to give a detailed account of the economy of wasps, which would occupy a volume, but merely to mention some statements of various authors respecting them, which have not been borne out by my own observations, I shall only give a slight outline of their habits, in order to make the details more intelligible. The interior arrangement of the nests of both species is so nearly alike, that one description will serve for both.

It can be hardly necessary to state that societies of wasps, as of bees, consist of three different classes of inhabitants, viz. males, females, and neuters. The females, which are much larger than the others, are familiarly known to every body as the large breeding wasps which appear in the spring. The neuters, or imperfectly developed females, are the ordinary wasps which infest our houses and gardens, and form the bulk of the colony. The males, not differing much from these in size or general appearance, are yet easily recognised by the practised observer, from the greater length of their antennæ, and their longer and more slender form: besides which, as is the case with the males in all this class of insects, they have no sting.

Early in the spring the female wasps, which alone survive the winter, and which have been impregnated in the preceding autumn, seek out a suitable situation for their nests, and proceed to form a few cells in which they deposit the eggs of *neuters only*. These eggs are often laid when the cell is in an unfinished state, in order that they may be hatched as early as possible, and proceed to assist the foundress of the nest in her labours. Two or more females never unite in forming a colony, but each nest is the work of a single individual.

The author of the Insect Miscellanies (p. 307) states, that "the female wasp, instead of producing "at her first laying the eggs of workers only, deposits those of *males* and *females*; but the latter, "when hatched and come to maturity, are only the "sixth part of the size of their mother, and only "lay the eggs of males." This statement seems to have been founded on some remarks of M. Perrot, a friend of Hubers [d], who supposed that there were two sorts of female wasps, the smaller of which were not larger than neuters, and which only laid the eggs of males; but he did not succeed in establishing this fact, which is entirely opposed to my own observation, as well as that of other persons who have practically attended to the subject. I have never found the eggs of males or females, early in the year, in any nest; and all the female wasps I have ever examined have been nearly as large as the foundress of the colony. Since the end of their creation is the reproduction of the species, and as the females do not lay eggs the same year that they are hatched, (according to most authors,) or take any part in the labours of the colony, they

[d] Vide Kirby and Spence, vol. ii. p. 108.

are *a priori* less likely to be hatched early in the year than the neuters, who are constantly employed in building and enlarging the nest, and in feeding the larvæ during the breeding season, and whose number far exceeds that of both the males and females put together; and it appears from observation, that the actual state of the case agrees with what we should have been led to expect.

The tree wasp frequently attaches its nest to a beam in a shed, or under the eaves of a house; but generally speaking it is found suspended from the branch of a young tree, or in a thorn hedge, and without much apparent pains being taken to conceal it. I have more than once seen a nest hanging over a walk in a young plantation, so that it could not escape the observation of any one passing that way; and one of the specimens now in the Museum was placed so near a gate leading into a wood, that it was hardly possible to go through it without disturbing the inmates. I have more often seen them attached to larch trees than to any other; but they are also found in the Scotch fir, the elm, and the beech, and still more frequently in gooseberry bushes in gardens, where they are likely to find abundance of food[e]. Professor Rennie, in the Insect Architecture, (p. 82,) observes, that "the tree which the "Britannic wasp prefers, is the silver fir, whose "broad flat branch serves as a protection both from "the sun and the rain." I have seen some hundreds of nests in young plantations, which abounded with silver firs, but I hardly ever saw a single instance of their being suspended from that species of pine. The larch, which they seem to prefer, affords less protection than almost any other tree; but from the nature of the materials of which the nest is composed, they are nearly independent of shelter either from heat or wet.

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[e] One of the specimens in the Museum has three holes in the top of it, in which large red gooseberries were growing at the time it was taken.

It was not till Reaumur had devoted many years to the study of insects, that he succeeded in discovering the nature of the materials employed by wasps in forming their habitations. It is unnecessary to repeat his interesting account, as it has been copied into almost every subsequent work on the subject[f]. He asserts that the wood which they prefer for that purpose is such as has been long exposed to the weather, and is old and dry. White[g], of Selborne, and Kirby and Spence[h], on the contrary, maintain that they make use of sound timber only. I have often observed wasps in the act of collecting materials from fir wood, but not from any other species, and this was never in a state of advanced decay, but nearly in the condition described by Reaumur. The paper of which the ground wasp forms its nest is more friable than that used by the other species, and some nests are of a much more yellow tint than others, and in fact resemble as nearly as possible the colour of rotten wood. It seems probable therefore that the ground wasp employs wood in various states of decay, but I believe the Vespa Britannica only uses wood nearly sound, on account of the greater degree of toughness requisite in a nest that is to resist the action of the weather. In such cases it gnaws the softer parts

[f] Reaumur, vol. vi. p. 182. [g] White's Selborne, vol. ii. p. 228.

[h] Kirby and Spence, vol. i. p. 508.

away, leaving the streaks or fibres standing up above the rest of the surface, being perhaps too hard to answer its purpose.

Mr. Wood, in the work before quoted, says, "The "*males* are employed to collect the wood from the "frames of windows, and from old posts and rails." If by this he means that the males are employed *exclusively* in collecting materials, (and he does not mention that the neuters give them any assistance,) the statement cannot possibly be correct, and I much doubt whether they ever join the others at all in that part of the labours of the colony. They are certainly not produced early in the season, when the first neuters are hatched; and at all events their numbers are so small compared with the neuters, that they would be quite incapable of collecting sufficient wood for constructing a nest of ordinary dimensions. I believe that their chief occupation consists in cleaning and preparing the cells for successive broods, though they may perhaps occasionally assist the neuters in building and gathering materials; but the chief portion of the work is performed by the latter; and we accordingly find, that they are the first that die in the autumn, when there is no longer any necessity for their assistance. Kirby and Spence (vol. ii. p. 110.) likewise deny that the males assist in building or in feeding the young, but consider them as the "scavengers of the com"munity, for they sweep the passages and streets, "and carry away all the filth."

The exterior covering of the nest of the Vespa Britannica, when completed, is composed of from ten to sixteen layers of the paper-like substance before mentioned, disposed one over the other, in such a manner that each sheet barely touches the next, instead of being placed so as to form a solid mass. In this state, a nest is capable of resisting the heaviest rains. In both species the layers of cells in which the grubs are deposited are formed of the same materials as the outside, and are always placed with the opening of the cells downwards, differing in this respect from those of the bee, in which the cells are always in a horizontal position. Both species too commence their work from the top, and build downwards.

The nest of the tree wasp in its earliest state does not exceed an inch in diameter, and contains five or six cells only. It is formed of two semicircular layers of the paper, the upper one projecting a little over the other, so as to shoot off the rain. A hole is left at the bottom of it just large enough to admit the female wasp. As soon as the first workers quit their cells, they begin the task of enlarging the nest, and of adding fresh layers of cells in which the female immediately deposits more eggs. The nest however in all its stages has the appearance of being in a *finished state* externally, and it is probably this fact which has given rise to a great many mistakes in authors, who from finding nests varying in size from one to twelve inches in diameter, have been led to describe them as the productions of distinct species of wasps, whilst they were in reality the work of the Vespa Britannica alone. It sometimes of course happens, that when the nest is in its earliest state, the female wasp is accidentally destroyed when absent from it. In this case the neuters forsake their habitation, and by some curious instinct the young wasps which emerge from their cells after the others have left it, immediately follow their example[i]. Specimens of such deserted nests are occasionally found in the later months; and this has contributed to keep alive the error that they were the work of some rare species, to which the names of Vespa Campanaria, from the bell shape of the early nests, and Vespa Holsatica have been given.

Ray mentions " a species of Vespa, which attaches " its nest to the beam of a building, three inches in " diameter." This I have no doubt was the nest of the Vespa Britannica, and was probably found about May, when they usually attain that size.

Reaumur figures a nest[k] which he says contained two layers of cells, and bore a considerable resemblance in size and shape to a half expanded Provence rose, and Kirby and Spence[l] mention a nest, which they consider to be of the same species, except that it contained only one layer of cells, as occurring in a garden at East Dale. It is called la Guêpe de Holstein, (Vespa Holsatica,) by Latreille[m]; but the descriptions given by these various authors agree precisely with that of the Vespa Britannica, to which species it must be referred. I am further confirmed in this opinion, from the fact that Reaumur's nest contained two layers of cells, and Kirby and Spence's only one. This shews, that however perfect the nest might appear externally, it was still in a state of progression, and that in both cases, had they been allowed to continue undisturbed, several more layers would have been added to each.

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[i] Reaumur, vol. vi.

[k] Reaumur, vol. vi. pl. 19. fig. 1, 2. The above figure is copied in Insect Arch. p. 82.

[l] Kirby and Spence, vol. i. p. 510.

[m] Annales du Museum d'Hist. Nat. i. 289.

Mr. Knapp, in his very entertaining work, the Journal of a Naturalist, (p. 333, 2d edit.) mentions that he had seen the nest of another rare British species of wasp, which he calls Vespa Campanaria. It is fortunately figured in his work, and I venture to give the same explanation of it as of the others. Indeed, on comparing a small nest of the Vespa Britannica with his engraving, the similarity is such, that a doubt can hardly exist upon the subject[n].

It seems that Mr. Knapp considers the Vespa Campanaria here spoken of, to be a distinct species from the Vespa Holsatica. Shaw, in the Naturalist's Miscellany[o], gives a figure of the nest of the "Campanular "wasp," to which he also applies the specific name Vespa Holsatica of Latreille. His engraving represents the nest in a somewhat more forward state than that of Mr. Knapp, and his figure of the wasp itself agrees in every respect with the Vespa Britannica. The account he gives of its habits is very vague, but he had evidently never seen the nest of a tree wasp in its mature state, as he goes on to say, "There is "an exotic species of wasp, a native of America, "which forms a nest on a plan *exactly similar* to "our present species, but differing in being much "larger, and having the concentric globes far more "numerous, viz. from ten to twelve or more. These "American wasps' nests are commonly suspended "from, or rather affixed to, the small branches of "trees, as firs," &c. Shaw was evidently not aware that these nests are common in Great Britain, and from his description I should infer that our species is identical with the American tree wasp, as may not improbably be the case, it being an inhabitant of so high a latitude. Mr. Stephens, in his catalogue of British insects, considers Vespa Campanaria to be identical with Vespa Holsatica, but distinct from Vespa Britannica, but even against so great an authority, I am inclined to think that further research will prove them to be all of one species.

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[n] Several nests of the Vespa Britannica, in various stages, may be seen at the Ashmolean Museum, and will convey a better idea of the changes they undergo, and of their similarity to the engravings referred to, than any description.

[o] Vol. xv. plate 603.

The most curious opinion however that has been stated with regard to these early nests is that of Leach[p], who has given a very correct figure of the Vespa Britannica, as well as of the nest in its early state. He says, "In the spring, the female constructs "a nest: the eggs in a short time produce larvæ, "which are fed by the parent until they arrive at "maturity, when she closes the mouth of the cells. "Towards the latter end of summer they change "into the perfect state, and this brood consists en-"tirely of neuters, who with the parent, construct a "*new nest* of larger size, in the cells of which the "female deposits the remaining portion of her eggs, "whose offspring are fed and brought up by the "neuters. This second brood is made up of males, "females, and neuters, and the latter inmates, either "unite their efforts to enlarge the nest, or construct "a new habitation, and some of the females live over "the winter. Thus far I have ventured to assert "on my own observation."—"The autumnal nests "are found suspended from trees, especially from "pines, varying from the size of a pear to a foot in "diameter."

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[p] Leach's Zoolog. Misc. vol. i pl. 50.

I have certainly never seen any thing tending to prove that wasps construct *two* nests in the year, and that they bring up part of their young in one nest, and then quit it, in order to undergo the labour of constructing a second, instead of enlarging the first; nor is there any analogy, in the history of any other insect, which would lead us to infer that such is likely to be the case. The fact, that the *same nest* is gradually enlarged from one to twelve inches in diameter, does not admit of a doubt, and a very slight degree of observation is sufficient to confirm it; and Leach is further in error, when he says that the first brood of neuters is not hatched till "late in the summer." It requires about thirty days[q] from the time the egg is deposited, till its transformation into a perfect wasp is completed; and as the nests are commenced early in the spring, it follows that the first brood must emerge from their cells only a month later. It is also contrary to the opinion of all authors who have written upon the subject, to suppose that the mother of the brood covers the grubs herself with that silky covering under which their last transformation is completed. Reaumur frequently saw the larvæ in the act of spinning the envelope, and I can confirm his account from the opportunities I have had of watching them. It appears therefore that Dr. Leach's statement is entitled to very little credit.

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[q] The egg is usually hatched in eight days, and then assumes the form of a grub; it is then fed by the female for thirteen or fourteen days, when the grub covers the mouth of its cell with the silky substance before mentioned. It remains in this state about nine days more, and then eats its way through the covering, and joins the rest in the labours of the nest. As soon as any neuters are hatched, the care of feeding the larvæ devolves upon them, and from that time the female rarely if ever quits the nest. On emerging from the cell, the young wasp is somewhat lighter coloured than the rest, but is the same in all other respects.

There is but one statement of Reaumur's, of any importance, which I have always found to be contradicted by my own experience; and upon this point I have the satisfaction of being able to bring forward the authority of Mr. Denison in my favour, who has paid great attention to the habits of wasps, and whose observations had also led him to a different conclusion from those of the French naturalist. It is with respect to the position that the larvæ of the *male* wasps occupy in the nest. Reaumur is speaking of the ground wasp, but his remarks apply equally to the other species. He says, "It is re-" "markable that the cells of the neuters are never "mixed up with those of the males or of the fe-" "males. One layer is composed exclusively of the "cell of neuters, but the cells of the male and female "grubs are often found in the same layer.—As the "larvæ of these two sexes are of equal length, it is "necessary for them to be provided with cells deep "enough to suit them. But the cells of the male "larvæ are narrower than those of the female ones, "because the males never become equally large in "diameter. I have often opened the cells, when the "inmates were nearly ready to emerge, and I have "always found that the cell contained either a male "or a female, as I had previously supposed it would "from its appearance." Kirby and Spence[r] also quote this statement. I have never found in any single instance a male larva in the cells appropriated to females, and Mr. Denison has come to the same conclusion after a careful examination of a great number of nests. I have repeatedly found male grubs mixed indiscriminately in the *upper* layers, which are always devoted to neuters, but have never seen an instance of the contrary. It is perfectly true, as Reaumur says, that the cells of neuter grubs are too short for the reception of those of males; but this defect is always remedied by the male grubs themselves, which lengthen the cells, so as to suit their own bodies, by spinning the silky covering of a convex form, which gives it the requisite size. The neuters, on the contrary, make the covering almost flat; and cells which have been used for rearing male grubs, are easily distinguished from the others, by their projecting beyond the general surface of the comb in a convex form. The lower layers of a nest, which are formed last, are occupied by females; and the difference in size of the cells, when compared with those of the upper layers, must be obvious to every one.

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[r] Vol. i. p. 507.

De Geer mentions, that in a small tree nest he found but one layer of cells, and that some of these cells were longer than others; probably, he says, for the reception of *females;* and he supposes the nest to be a perfect one of some new species of wasp. The probability however is, that, as in the other instances above mentioned, it was merely an early nest of the Vespa Britannica, of which one layer only was completed. The cells in a layer often differ a little in length from each other; and if any of them in this case were so much longer, as to make it probable they were not intended for neuters, which is somewhat doubtful, I should suppose that they were prepared for the reception of *male,* rather than *female* grubs, because the latter would require them to be *wider* as well as longer; which De Geer does not say was the case.

The following notes were made on examining several nests of both species taken in the summer of 1833; and I have inserted them, to shew, in opposition to Reaumur, that male grubs are frequently found in the cells of the upper layers, which belong to the neuters.

No. 1. July 27th. A small nest of tree wasps.
1st layer contained none but neuters.
2d do. Neuters and males.
3d do. Females.
4th do. Only just begun, and not ready to receive the eggs.
Males and females are hardly ever hatched so early in the summer, and none but neuters were found in the nest.

No. 2. July 24. Tree wasps.
1st and 2d layers. Neuters with a few males amongst them.
3d and 4th do. Females not hatched.

No. 3. July 27. Tree wasps.
1st and 2d layers. Neuters with a few males.
3d and 4th do. Females.
5th do. Just commenced.

No. 4. August 6th. A large ground nest.
1st, 2d, 3d, 4th, and 5th layers. Neuters with a few males in each.
6th and 7th do. Females only.

No. 5. Aug. 7. Ground wasps; a large nest.
1st and 2d layers. Neuters only.
3d do. Neuters and males.
4th do. Neuters only.
5th do. Neuters and males.
6th and 7th do. Neuters only.
8th, 9th, and 10th do. Females only, the larvæ not far advanced.

No. 6. Aug. 8. Ground wasps.
1st layer. Neuters with one or two males.
2d do. Neuters only.
3d do. Neuters and males.
4th, 5th, 6th, 7th, do. Neuters only.
8th do. Females only.

Similar results were obtained from the examination of a great many other nests. I found no instance of a male grub in the female cells, which Reaumur states to be constantly the case, and am unable to offer any explanation of the disagreement between his observations, and those of Mr. Denison and myself. It is possible that Reaumur's descriptions were not founded on the inspection of a great number of nests, as he usually refers to one only which he had succeeded in transferring to his own house. Had he examined a greater quantity, and at different times in the summer, he could not have failed to observe the male grubs mixed indiscriminately amongst the neuters. At the same time I must own that further investigation may be expected to shew, that males are sometimes found in the female cells; as I cannot think that so acute an observer as Reaumur could have been mistaken, when he so positively asserts that fact on his own authority.

The beautiful arrangement by which the layers in the nest are attached to each other, so as to allow room for the wasps to walk between them, will be easily understood by examining any of the specimens in the Museum. In the ground nests, the supports or braces are round, like small columns, and dispersed at irregular distances. The upper end is spread along the edges of three cells, so as to divide the pressure, and yet allow room for the grubs to work their way out, when they are come to maturity. In the tree nest a somewhat different arrangement is adopted. Instead of round pillar-like braces, thin slips of the paper of which the whole nest is composed, but made stiffer for this purpose, are continued along the edges of a number of cells, so as not to interfere with the inmates, and are finally fixed to the layer below. The number of these fastenings varies in every layer.

In examining the cells of some of the nests, a difficulty occurs of which I can offer no very satisfactory explanation. The cells of the uppermost layers are in some instances gnawed away, so as to be much too shallow for the reception of a grub. They all bear the marks of the teeth of the wasps, and are not eaten down with much regularity. The nests in which this peculiarity occurred, were taken about the middle of August, and I can only account for this appearance by supposing that at that time of year no more neuters were necessary for carrying on the works of the nest; and as any eggs deposited then, could not be matured before the middle of September, the cells were no longer wanted, and were gnawed away to give more room to the inmates. The female cells, being still occupied by the young in their various stages, were left entire. The larvæ that remain in the nest when the cold weather sets in, are always destroyed by the survivors, who find themselves unable any longer to procure food for them. The males are chiefly employed in this work, and may sometimes be seen dragging the grubs to the outside of the nest, where they are left to perish.

The females leave the nest in October, and seek out for winter quarters, in which they remain in a torpid state till the warmth of spring again revives them. They conceal themselves in decayed stumps of trees, and under masses of moss and dead leaves, or under the tiles or slates of the roofs of houses. I dug out several during the last winter, and very frequently found humble bees in the same situations. The males and neuters die without exception, as soon as they have fulfilled the great end of their creation, and nearly all the authors assert that only a few females survive the rigour of the winter. This assertion is certainly difficult to be proved, and its truth has been inferred from the deficiency of wasps in some seasons, when they had been particularly abundant the year before. In the summer of 1832 I could not find a single tree wasp's nest, and only two or three of the ground species; but I conceive that the failure of wasps in such seasons depends more on the rigour of the spring, which often benumbs and destroys the females which had been tempted to leave their winter retreat too early, than on their being unable to resist the frosts of winter, when they are in a torpid state, and less likely to be affected by cold. I have observed great numbers of hive bees destroyed in a cold wet spring from the same cause, as well as other insects which had not the shelter of a hive to protect them, but which had nevertheless survived the frost of winter in a torpid state. Wasps are found to live through a Norwegian winter, and therefore no frost that occurs in this climate is likely to affect them[s].

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[s] Mr. Knight supposes that when a large number of females has appeared in the spring, and very few nests are found in the summer months, the failure proceeds from a deficiency of

Kirby and Spence[t] say, that when the winter sets in, "the nest merely serves for the abode of a few "benumbed females, and is entirely abandoned at the "approach of spring." I have had frequent opportunities of opening nests during the winter, but have invariably found them empty, and have reason to believe that no females use them as a hybernaculum, but rather prefer seeking a retreat for themselves in the situations already mentioned.

Some writers have doubted the assertion that a sentinel is placed at the entrance of the nest, as soon as the wasps retire to rest in the evening; but I can state most positively that I have never seen a nest of either species, after about nine o'clock in the summer months, without observing the sentry at his post; and it is curious to see with what rapidity the alarm is conveyed to those within, when any attempt is made to meddle with him. I have sometimes thought that I could see a second sentinel at some distance behind the outer one; and judging from the usual distance from the entrance down to the nest, (often from two to three feet,) and the quickness of the communication with it, there may even be more. A lantern held near the sentinel does not seem to give him any alarm; but on touching the ground near him, he instantly disappears for a few seconds, and the inhabitants sally out immediately. I always capture the sentinel before proceeding to take the nest, and they are invariably neuters. A consider-

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males in the preceding autumn. The explanation I have offered seems a more natural one.—Vide Philosoph. Trans. for 1807, p. 243.

[t] Vol. i. p. 509.

able number of wasps pass the night upon the outside of the tree nest, in warm weather, but the sentinel is always to be seen at the entrance notwithstanding. It may be as well to mention, that in the ground nests there are two apertures, one for entrance, and the other for exit. The tree nest has usually only one, near the bottom; but in large colonies a second is often added; in which case there is a sentinel stationed at each.

There is a curious fact with regard to wasps, which, though already published, it may be worth while to mention, as I have had many opportunities of verifying it from experience; viz. that if the entrance to the nest be stopped up in the day time, the hundreds of wasps which are constantly returning to it make no attempt to sting the aggressor; but if one escape from the inside, it attacks him instantly, though not with the pertinacity of the common bee. I have often suffocated the wasps in a nest with a mixture of gunpowder and sulphur, in the day time, and proceeded to dig them out directly, whilst hundreds were flying about me in all directions, but I have hitherto escaped with impunity. If a common quart bottle half full of water, is placed in the cavity, and the ground levelled up again, so that the mouth of the bottle may as nearly as possible occupy the place of the former entrance, all the wasps that were absent at the time of the taking of the nest, will crowd into the bottle during the day; and I have usually found from 500 to 1500 wasps captured in it on the following morning. It is interesting to observe the young wasps eat their way through the covering of the cells when their transformation is complete. This they will continue to do for several days after the nest is taken, particularly if kept in a warm place.

There are many curious circumstances connected with the habits of wasps, their mode of obtaining food, &c. which it is not the object of this paper to detail, because, as far as I can judge, they are correctly stated in all the works upon the subject.

I shall only refer to one other point in their natural history, and that is with regard to certain insect enemies to whose attacks they are exposed. There is a species of Volucella, the grub of which is figured in Kirby and Spence's work[u], and there described as inhabiting the nests of humble bees. It has also been found by Mr. Denison in those of wasps; but whether it feeds on the comb or the young wasps I do not know. Leach also speaks of wasps as being much infested by a small coleopterous insect called Lebia Linearis.

The active hostility which is carried on by the ichneumon tribe against the insect world in general has attracted the notice of all entomologists. But the social hymenoptera, the ant, the bee, and the wasp, have been said by the older naturalists to enjoy an exemption from the attacks of these formidable enemies: and the same statement has been repeated of late in the popular work of Messrs Kirby and Spence. "No ichneumon," say these authors, "has been yet found to deposit its "eggs upon the larvæ of the ant, the wasp, the "humble bee, or the hive bee, in which last, had "there been one appropriated to it, it could never "have escaped the notice of the Reaumurs and the "Hubers." As regards the wasp, however, it seems that this exemption does not exist; for though I have not myself been so fortunate as to find any species of ichneumon in their nests, one has been seen by Mr. Denison in several instances, and observed in all the stages of its growth. It is described by him as a fly as large, or nearly so, as the wasp itself: the head and forepart of the body black: the abdomen yellow, with a dark streak down the back: legs and under wings black: upper wings dusky. This fly deposits its egg upon the grub of the wasp at the moment it assumes the pupa: as soon as the egg is hatched, it devours the grub of the wasp entirely, and itself assumes the pupa and imago form in the cell of the wasp. As it is always found in cells closed up by the grub of the wasp in order to the assumption of the pupa state, it is probable that the parent fly may pierce the covering of the grub with its ovipositor, and lay its egg in that manner, the hole made in the covering being so small as to escape notice. These flies are most likely to be found where single cells are seen closed, after the inhabitants of the adjacent ones have quitted them; because the ichneumon has to go through three stages, while the wasp has only to pass the pupa state, supposing the grubs in the same layer to be nearly of an age at the time they were deposited. I am not aware that the existence of this ichneumon has ever been mentioned by any writer on entomology, nor was any other described till some time after this one had been observed by Mr. Denison; but a similar discovery has since been made by Mr. Wood of Manchester, who observes, "In examining "the combs of some wasps' nests, in one of them I "discovered many cells, about half the length of

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[u] Vol. i. p. 264; ii. p. 223, and Plate xix. fig. 11.

" those of the wasps, and capped with wax[v]. I put " the comb into a glass jar, and the day following " had the gratification of finding that they had eaten " their way out of the cells." Mr. Wood has called this species of ichneumon, Anomalon Vesparum; it differs considerably from Mr. Denison's, and is much smaller. Mr. Curtis afterwards saw the cells containing its young, and says, " It is worthy of remark, " that the cells of the wasp containing the anomalon, were closed like the others, and upon opening them, the exuviæ of the wasp's grub filled up a " space of about one third of the cell, from which we " conclude that the eggs were deposited in the bodies " of the larvæ, and lived upon them till they became " nymphæ."

These late discoveries, as well as the many instances of disagreement and probable error, which I have pointed out, in the accounts both of the older and more recent writers upon the natural history of wasps, shew that there is still much room for further observations upon their habits; and I hope that some other members of the Society may be induced to carry on these investigations, so as to clear up any doubts which still exist.

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[v] I conclude that the covering of the cells of the ichneumon was not made of *wax*, but of that same silky substance in which the larvæ of other insects of that class envelope themselves.

REMARKS

ON THE

PROPORTIONATE QUANTITIES

OF

RAIN

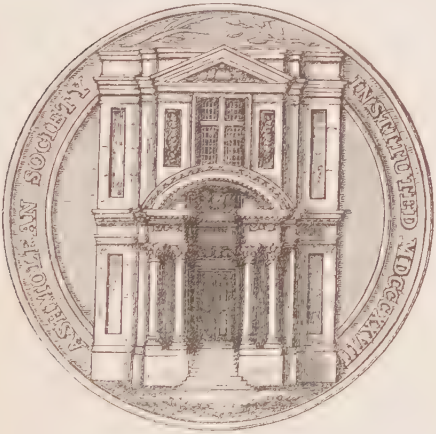
AT DIFFERENT SEASONS

IN OXFORD.

BY

STEPHEN PETER RIGAUD, M. A.

SAVILIAN PROFESSOR OF ASTRONOMY.



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ON

THE PROPORTIONATE QUANTITIES OF RAIN AT DIFFERENT SEASONS IN OXFORD.

READ TO THE ASHMOLEAN SOCIETY, MARCH 13, 1835.

THE quantity of rain and the particular seasons of its fall form an essential part of observations in meteorology. The subject is connected with the comforts and convenience of life, and involves the more important considerations of its effects upon the productions of the earth. The state of the atmosphere, which disposes it for the absorption or discharge of moisture may depend on chemical affinities with which we are as yet unacquainted; but the variations in respect to density, electricity, and temperature are well known causes of the phenomena in question. These have their peculiar effects from the relative state of those different parts of the earth's surface with which the air may come into contact, and consequently the weather must always in great measure depend on the quarter from which the wind at the time is blowing. All the other causes here enumerated, if not dependent on, are greatly modified by, heat and cold: between the tropics, therefore, the overwhelming influence of a vertical sun produces in many places a regularity, which we, in higher latitudes, have no opportunity of witnessing. Near the equator, the rainy seasons often return, not only at the same precise times of the year, but nearly with the same degrees of intensity. With us, at a distance from the torrid zone, this is not the case: our situation likewise on an island (too large not to have its own peculiar influence, and yet not so extensive as to overbalance the effects of the neighbouring continent) materially affects the alternate predominance of those conflicting elements which determine the state of the weather. The fall of rain is also influenced more than is generally imagined by local circumstances. Besides the more general causes, elevation or depression will affect it: particular soils will radiate their heat in greater or less abundance, and consequently may condense the atmospherical vapour in different quantities: thunder storms are well known to be more frequent in some places than in others: and a chain, not merely of mountains, but of hills, will make a decided difference in the rain which will fall on either side of it. The due proportional effects of such varied causes can only be estimated by long and constant attention to the phenomena which they produce; but there can be no doubt that patient investigation will eventually lead us to a general knowledge of their several influences. If our climate is proverbially changeable, it has been exempted, by the blessing of Providence, from the catastrophes which in some countries return with dreadful frequency. We are not tempted to exclaim,

> But errs not Nature from its gracious end,
> From burning suns when livid deaths descend,
> When earthquakes swallow, or when tempests sweep
> Towns to one grave, whole nations to the deep?

and if we are surrounded by phenomena whose intricacies may be difficult to unravel, still the same answer applies to any doubt which they may occasion;

. . . . the first Almighty Cause
Acts not by partial but by general laws;

for the analogy of nature forbids us to imagine that there is no mutual compensation between the causes by which they are governed. This compensation may require a long period for its developement, but its existence might be assumed *a priori*. It is not however intended to invert the legitimate order of investigation by entering further on this part of the argument. It is too early to speculate on causes, when their effects have not been duly established; and all that will now be attempted is a simple digest of facts, with which other observations can be compared, so that the truth may be elicited by further confirmation, or by the modifications and correction which are found necessary.

The registers of the rain which have been kept for many years at the Observatory, afforded the means of trying whether any empirical conclusions could be fairly deduced from them; and the quantities for the last twenty years were therefore taken out as they are inserted in the annexed table. It contains the quantity for each month of the respective years, reckoned, as is usual, according to the number of inches to which the depth would amount perpendicularly upon any given surface. The copper funnel, in which the rain is received, is of a conical form, 10½ inches in depth, and with a circular mouth exactly a foot in diameter. The uppermost surface stands 22 feet 9 inches above the surrounding ground, and is raised a little higher than the adjoining roof and walls of the eastern wing of the Observatory on which it is fixed.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Total.	
1815	0.585	0.728	2.027	1.194	2.136	2.060	0.769	1.592	2.448	2.462	0.685	1.943	18.629	1815
1816	2.078	1.242	1.191	1.847	1.357	1.434	4.014	2.261	1.500	2.858	1.695	2.851	24.328	1816
1817	1.693	0.847	1.115	0.144	2.745	2.044	2.563	3.365	0.311	0.654	1.557	2.222	19.260	1817
1818	2.380	1.481	2.499	3.903	2.275	0.193	0.327	0.324	3.654	1.707	3.512	0.822	23.077	1818
1819	1.607	2.449	0.865	1.148	2.725	1.876	1.214	0.566	3.476	1.713	1.545	1.972	21.156	1819
1820	1.879	0.482	0.273	2.115	2.310	0.889	1.691	0.818	1.822	1.885	1.795	1.798	17.757	1820
1821	1.802	0.087	1.876	1.973	1.193	1.130	1.824	3.216	3.407	2.002	3.172	3.428	25.110	1821
1822	0.686	0.942	0.722	2.097	1.487	0.305	3.953	0.120	1.946	2.394	3.506	1.083	19.241	1822
1823	1.360	2.645	1.055	1.873	1.251	1.166	1.861	3.314	2.320	3.470	1.444	2.870	24.629	1823
1824	0.462	2.063	1.558	2.790	1.060	3.070	0.985	3.310	1.914	3.744	3.166	2.527	26.649	1824
1825	0.000	1.362	1.250	1.402	2.594	1.583	0.030	2.977	2.304	2.262	1.528	2.136	19.428	1825
1826	1.147	1.346	1.668	0.964	1.143	0.388	0.705	1.093	3.966	2.516	1.246	1.604	17.786	1826
1827	0.928	0.721	2.007	0.736	1.499	1.508	1.403	1.691	3.262	4.249	1.651	2.574	22.229	1827
1828	2.655	0.817	0.401	2.485	1.841	1.896	3.520	2.674	2.434	2.028	1.041	1.944	23.736	1828
1829	0.547	1.137	0.200	3.326	0.143	2.846	3.620	3.447	2.545	1.459	0.954	0.402	20.626	1829
1830	0.774	1.120	0.331	2.449	3.369	3.194	2.696	3.516	4.058	0.763	1.908	1.129	25.307	1830
1831	2.037	2.046	1.861	1.414	2.065	1.365	3.978	1.787	3.445	2.812	2.032	2.634	27.476	1831
1832	1.347	0.329	1.711	2.089	2.507	2.660	1.417	4.104	0.920	2.944	2.694	1.598	24.320	1832
1833	0.620	3.837	0.971	2.013	0.271	2.263	1.970	1.075	1.152	2.411	1.807	3.109	21.499	1833
1834	2.325	0.442	0.372	0.533	0.981	1.024	6.473	1.760	3.149	0.920	1.208	0.722	19.909	1834
Mean	1.294	1.352	1.241	1.893	1.788	1.677	2.028	2.171	2.468	2.333	1.944	2.034	22.223	19 years.[a]
	1.346	1.306	1.198	1.825	1.748	1.645	2.251	2.150	2.502	2.263	1.907	1.968	22.109	20 years.[b]

[a] From 1815 to 1833.

[b] From 1815 to 1834.

A period of five or even of ten years was found to be insufficient for any general result, but fifteen years began to lead to conclusions which had a certain degree of uniformity, and appeared likely to afford a clew for further investigation. With this view, however, it seemed right to omit the year 1834: it was a most anomalous season; not perhaps so much from the small quantity of rain which fell in it, as from so very large a proportion of the whole having fallen within limited periods. The effect will be shewn that this produces on the general averages, but in the computation of them it has been thought better to confine the attention to the preceding period of nineteen years from 1815 to 1833 inclusive. This will possibly be sufficient for a first approximate induction, and it has the advantage of being the same with the golden number, or Metonic Cycle, at the end of which the new moons return again in succession, so as to coincide with the same days of the several months. There is a general impression that the lunar influence is very great on the state of the weather, and some have even imagined that a knowledge of the moon's phases is sufficient to ascertain what may be expected. This however is certainly assuming too much. Dr. Horsley drew up an abridged state of the weather at London in 1774, in which he speaks with very great hesitation of this alleged agency[a]: and although he found in the following year that the trial, as he says, "turned out more in favour of the moon," still the changes were even then "many more on "the days confessedly exempt from her influence, "than on those which have been supposed to be most subject to it[b].” In the collection, however, and arrangement of facts, it is advantageous to have the power of investigating the manner in which they may bear upon this question.

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[a] Phil. Trans. Vol. LXV. p. 178.

The absolute quantities will not lead to just conclusions without they are considered with reference to the general character of the seasons in which they occur. Thus in June 1824 we had 3.070 inches of rain, and in the same month of 1829 we had 2.846; but the latter is the greater proportionate quantity when compared with the whole of that which fell in those respective years; for, $\frac{3.07}{26.649}=0.115$. and $\frac{2.846}{20.624}=0.138$. To simplify the proportions therefore, the annual totals, or their sums, (when several years are taken together,) have in making comparisons been considered as consisting of 100 parts, so that the numbers used may express for each portion the quantity of these hundredths which may belong to it. If all our months were of equal length, and there were no bissextiles, their mean value would be 8.33, and this will be sufficient for a general estimate: the limits to which the error can extend is not great, the most considerable of course would be for February in a common year, which is only 7.67 of the whole, which does not make a difference of more than 0.66 or $\frac{2}{3}$ of a single hundredth. For a month of thirty days in a leap year the number is 8.20, and for a month of 31 days in a common year, it is 8.49. This however, even if it were more considerable, would not affect the present inquiry, which is only directed to the quantities of rain which fall in

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[a] Phil. Trans. Vol. LXVI. p. 363.

each successive month of the civil division of the year.

Before the more general examinations shall be made, it may be useful to consider some of the particulars which are offered by the respective months.

January is the only month in which there has been an instance of no rain having fallen during the nineteen years which have been already specified; and to save repetitions, it may be right to premise, that, unless notice is taken to the contrary, all the conclusions must be understood to be limited to this interval. This remarkable dryness occurred in 1825; and in 1815 and 1824 less rain fell in January than in any other month of the year. In 1828 it gave 11.12 parts of the whole, but this is its maximum; it has not proved the wettest month of the year, and indeed its average is much below the twelfth part of the whole annual fall.

February in 1821 gave only 0.35; and it was then, as might be imagined, the driest month of the year: it was so also in 1827 and 1832. Its average is rather higher than that of January, and it seems liable to greater fluctuations. In 1833 it was the wettest month of the year, its quantity of rain amounting to 17.85 parts. In other years it has been a wet month, though not to the excess of 1833; but after all it cannot claim with us the designation, which it has obtained in some other parts of the country, of February fill-dike.

March again upon the whole has been a dry month: it has indeed been oftener than any other the driest of the year, having proved so in 1816, 1820, 1823, 1828, 1830. It is an old saying that a peck of March dust is worth a king's ransom, and in this respect the season seems favourable to the agriculturists in the district which surrounds us. In no instance has March, although it consists of one and thirty days, brought down more rain than any other month of the year. The extremes have been 0.97 in 1829, and 10.88 in 1815, making a variation of 9.91.

April is usually a month in which short heavy rains, and sometimes hail storms, occur, when in the intervals the sky exhibits bright sunshine. Its average has been higher than that of the preceding months. In 1817 it was the driest of the year, affording only 0.75; and in 1818 it was the wettest, producing 16.91. These are indeed the only instances in which it has been in the extremes of any years, but the variation amounts to 16.16.

The average for May is a little less than that for April. In 1829 it gave only 0.69. In that year, and in 1833, it was the driest month of the year, and in 1820 it was the wettest. In proportion however to the whole of the annual fall the largest quantity of rain fell in May 1817, which gave 14.25. The variation therefore amounts to only 13.56.

June, upon an average, has been one of the drier months, and has in no instance contributed more than any other to the annual fall of rain: it produced less than any in 1818, 1826, and 1831; its extremes have been 0.84 in 1818, and 13.80 in 1829, making a variation of 12.96.

July has certainly been a wet month, though not upon the whole in so large a proportion as seems generally to be believed. In 1825 only 0.15 was produced by it, but it has not been the driest month of any one year. In 1816, 1822, 1828, 1829, 1831, it was the wettest: in 1822 no less than 20.54 fell in this month; in 1834, however, the quantity was much larger, being 32.51, or nearly ⅓ of the whole rain which fell in the year. It is this extraordinary quantity in what was, even including it, still a dry year, which seemed to make it advisable not to take such a season into the general account. It appears indeed to have been characteristic of this year, that although the whole rain was considerably below the annual average, yet that when it did fall it was exceedingly heavy. Thus we had in

January	11.68
July	32.51
September	15.82
	60.01

So that no less than ⅔ of the annual fall took place in these three months, and 48.33, or nearly one half, in the two last. The variation indeed in July has amounted to 20.39, without taking into account the very great fall of 1834.

August, like July, has had more than its proportionate share of the annual rain. In 1822 however it only produced 0.62, and then, as well as in 1819, it was the driest month of the year; but in 1817, 1825, and 1832, it was the wettest, the rain in the first of these years amounting to 17.47. The variation therefore between the extremes was 16.85.

September has been decidedly our wettest month: in no instance has it produced less rain than any other in the same year: its least quantity was 1.61 in 1817; but in 1826 it produced no less than 22.30; and in that year, as well as in 1819 and 1830, it gave more than any other. The difference between the extremes is 20.69, which is a little more than that in July, when we omit 1834.

October has had the highest average next to September. It has not proved in any instance our driest month, its least proportionate quantity having been 3.01 in 1830; but it was the wettest month of the year in 1827, (when it produced 19,11,) and also in 1815, 1823, 1824. The extremes differ 16.10, which is less than in July and September.

November has produced rather more than a mean quantity of rain; but not by any means to the amount which is commonly imagined. It has not in any year been the extreme month either for drought or wet. The least quantity was 3.68 in 1815, and the largest was 18.22 in 1822, which gives a range of 14.54.

In December the average rises again, and it has once (in 1821) been found in the extreme of humidity for the year; but it has not in any instance been the driest month; the least quantity of rain was 1.95 in 1829, and the greatest 14.46 in 1833; the difference between which quantities is 12.51.

If now the averages of the whole nineteen years are tabulated they will give the following results:

January	5.82	July	9.13
February	6.08	August	9.77
March	5.58	September	11.10
April	8.52	October	10.50
May	8.05	November	8.75
June	7.55	December	9.15

From hence it appears that the three first months, of January, February, and March, have not upon the whole differed much from each other, and have been decidedly the driest of the year, the total rain which has fallen in them amounting only to 17.48, or little more than $\frac{1}{6}$ of the annual fall. It has been shewn likewise that out of nineteen years, the driest month has occurred three times in January, three in February, and five in March; and there is only one instance (of 1833) in which either of these three has produced more rain than any other of the twelve months.

The next three months, of April, May, and June, again have approached each other, so that they may be classed together, although their mutual differences have been rather greater than those of January, February, and March. They have averaged together 24.12, or rather less than $\frac{1}{4}$ of the whole year. It is found indeed that as we advance, the rain upon the whole has increased, though it is remarkable that the changes are sudden; the difference between March and April is relatively very considerable. After April, the quantity of rain gradually has diminished till the end of June. This is so much the case that we have not had the maximum for the year in this last month, although it has occurred once in April and once in May. On the other hand, the minimum has been three different years in June, twice in May, and once in April.

The quantity has gone on increasing through July, August, and September, and then begun again to diminish. July and August are the months in which thunder storms are most common, and although these are frequently of very limited extent, yet the quantity of rain which falls during their continuance is very great: this will account for the actual quantity at this season often very much exceeding the common average; but it is clear that the three months of August, September, and October have contributed more than any other equal portion of time to the sum total of the year: the average for them is 31.37, or nearly $\frac{1}{3}$ of the annual fall. July ranks more with November and December, and the sum of the three has been 27.03, or rather more than $\frac{1}{4}$.

Of the nineteen years which we are discussing, July has been the wettest month in five, August and September each in three, and October in four. There are only four single instances in which the greatest fall has been in February, April, May, and December.

The sudden increase from March to April is not found to have been occasioned by the storms which sometimes follow the equinox; and there is a much more remarkable change from the month of December, which has produced rather more than the mean quantity of rain, to the diminished fall which has taken place in January: this would seem to connect the less and greater quantities with the increasing and shortening of the length of the days, and the rain of the first six months has been to that of the last six as 41.60 to 58.40; but on the other hand it is to be noticed, that the relative effects of the sun's influence cannot be the only cause on which these phenomena depend, for the rain has been not only more abundant in April, May, and June, than in January, February, and March, but has diminished in October, November, and December, when the days are gradually approaching to their shortest duration.

This proportion will be sufficient for a division of the year into two nearly equal parts, because the last six months contain only three days more than the first: but there are some sources of uncertainty in endeavouring from our present observations to form an estimate of the rain which falls in the interval between one of the equinoxes and that which succeeds it. The sun is about eight days longer in passing from the vernal to the autumnal, than it is in returning from the autumnal to the vernal equinox. This might be allowed for; but it is not so easy to ascertain the effect which belongs to the days which intervene between the times of the equinox, and the ends of the months in which it occurs. These intervals, however, are not very different from each other; and if we take the rain which falls from the beginning of April to the end of September, and from the beginning of October to the end of March, the ratio of the one to the other has been as 54.12 to 45.88. If we take the months of the civil year, there are no six in succession which have given exactly the half of the annual rain: the nearest have been from the beginning of March to the end of August, and from the beginning of September to the end of February; but the former has been less than the latter in the ratio of 48.60 to 51.40.

The months arranged according to their relative humidity will give the following proportions:

Fifteen years,		Nineteen years,		Twenty years,
1815-1829	1819-1833	1815-1833		1815-1834
11.53	11.57	September	11.10	11.32
10.94	10.88	October	10.50	10.23
9.51	10.00	August	9.77	9.73
9.32	9.14	December	9.15	8.90
8.81	9.16	July	9.13	10.18
8.80	8.75	November	8.75	8.63
8.65	8.57	April	8.52	8.25
7.96	7.56	May	8.05	7.90
6.92	7.76	June	7.55	7.44
5.67	6.35	February	6.08	5.91
6.12	5.30	January	5.82	6.09
5.78	4.97	March	5.58	5.42

From this table it will be seen that there has been a certain order, which probably is of no casual description; for it obtains in general in the various periods which have been calculated for the results which are here arranged by the side of each other. There are indeed some partial discrepancies in the first three columns, but they are not more than might be expected in the present state of the inquiry. In the first fifteen years the average of November was a little above that of July in the ratio of 8.8052 to 8.7996, but in the second fifteen, and in the longer period of nineteen years, July has the larger quantity. The three first months of the year, though decidedly the driest, have not exactly preserved their relative numbers. March has not in either instance produced so much wet as January; but the fluctuations which have been noticed in February have placed it during different periods above and below the other two.

Although the anomaly of 1834 made it seem right to omit it in taking the general averages, the results of the twenty years have been added in the last column. This may not only be useful in justification of the omission, but may be satisfactory by shewing the extent to which it affects the conclusions. It will shew that no new view is opened by it in the order of the months, but from the dislocation of July, which is raised above August and December. The very circumstance of the differences being so very partial, which has been already explained, formed a reason for not taking it into the present consideration. In a longer period, when similar anomalies may have occurred in other months, this year may again be brought into the general calculation.

From the impossibility of as yet ascertaining each particular cause of change, the inquiry must be exposed to similar difficulties, which may hereafter be the very means of leading to otherwise-unsuspected truths. At first we are only able to take our position on chances, the conclusions from which will very often in particular cases differ widely from the facts, but still are the best means by which we can advance in generalizing our views of natural phenomena.

In consequence of what has been said respecting 1834, it seemed well to subjoin the general results of the observations for that year on the heights of the Barometer and Thermometer.

	Barometer.				Thermometer out of doors.			
	10 BN	2 AN	10 AN	Mean	10 BN	2 AN	10 AN	Mean
January	29.57	29.56	29.57	29.57	45.1	46.7	44.4	45.4
February	29.99	29.98	29.98	29.98	41.8	45.7	39.7	42.4
March	30.08	30.05	30.05	30.06	46.4	50.3	42.1	46.3
April	30.03	30.00	30.01	30.01	49.0	53.1	42.2	48.1
May	29.87	29.89	29.90	29.89	59.7	64.5	51.3	58.5
June	29.87	29.85	29.85	29.86	65.2	68.5	56.2	63.3
July	29.84	29.82	29.83	29.83	66.8	69.9	59.9	65.5
August	29.77	29.76	29.77	29.77	66.0	68.9	57.8	64.2
September	29.95	29.93	29.94	29.94	60.5	65.3	54.3	60.0
October	29.92	29.90	29.92	29.91	53.1	56.9	48.3	52.8
November	29.81	29.78	29.79	29.79	44.8	47.3	41.5	44.5
December	30.16	30.14	30.15	30.15	42.1	44.6	40.3	42.3
Mean	29.91	29.89	29.90	29.90	53.4	56.8	48.2	52.8

Extremes { Barometer 30.49 Dec. 11 and 15. 28.85 Jan. 12.
Thermometer 82. 8 June 21 24. 2 Dec. 24.

NARRATIVE

OF AN

EXCURSION TO THE LAKE AMSANCTUS

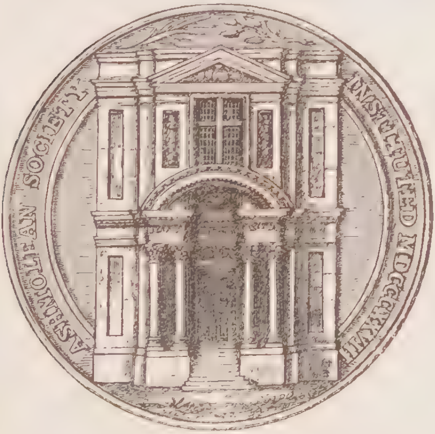
AND TO

MOUNT VULTUR IN APULIA

IN 1834.

BY

CHARLES DAUBENY, M. D. F. R. S.
PROFESSOR OF CHEMISTRY AND BOTANY.



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NARRATIVE

OF

AN EXCURSION TO THE LAKE AMSANCTUS AND TO MOUNT VULTUR IN APULIA, IN 1834.

READ TO THE ASHMOLEAN SOCIETY, DEC. 4, 1835.

WHEN we consider the rambling propensities of our countrymen, who every year since the peace have peopled at certain seasons the great roads of the continent, creating a new class of inns for their express accommodation, and introducing their own usages and fire-side comforts into the most distant extremities of Europe, it is singular, how few ever deviate from one or two beaten tracks, and what a scanty measure of information is doled out to us, with respect to places, that lie only a short distance apart from those lines, along which the tide of modern travelling is accustomed to flow.

So long as our inquiries, whether they chance to be of a scientific or of a literary description, are limited to a certain established range of country, we are absolutely embarrassed by the multitude of guide-books and travels that recommend themselves to our notice, and find ourselves provided with more minute instructions, as to the objects of curiosity existing in many parts, for instance, of Italy, than we are with respect to those of our own country.

But when we wish to step a little out of the beaten path, and desire to explore those parts of the continent, which, though lying neither in the direct road to Vienna, to Naples, or to Switzerland, are yet not without their own peculiar sources of interest, we often find it difficult to ascertain even the commonest particulars, with respect to the inhabitants, their roads, or their accommodations; and regard the most scanty and jejune account, which we may chance to discover, respecting such places, in the light of an important acquisition.

It is with this feeling, that I now offer to the Society a brief sketch of a tour, which I performed last winter through a part of Apulia, which, although abounding, not merely in curious natural phenomena, but likewise in picturesque beauties, such as at a better season would render it highly attractive to every lover of bold and romantic scenery, has scarcely to my knowledge been described in any of the many books of travels that have issued from the British press since the peace of 1815.

Mr. Fuller Craven, indeed, in his Tour through the southern Provinces of the Kingdom of Naples, has noticed a portion of the road; but he has almost entirely omitted mention of the two points which above all others attracted me to that country, I mean the Lago d' Ansanto and the extinct volcano of Mount Vultur; and sir R. Colt Hoare, who published his Classical Tour with the intention of supplying those blanks which existed in the descriptions given of the interior of Italy, was prevented, by the inclemency of the season, from proceeding further on the road to Apulia than Benevento.

In order therefore to render my Narrative of some use to those, who may be induced, though with different views to mine, to visit, the classical country of the Samnites and the Hirpini, the birthplace of Horace, or the field of Cannæ; all objects that might be readily comprehended in a tour similar to one I accomplished; I shall on the present occasion, not merely state the results of my inquiries into those natural phenomena which lay in my way, but throw the whole into the form of a journal, extracted, with some few additions, from the diary made on the spot[a].

On Wednesday, December 3, 1834, having engaged a Neapolitan Vetturino for the whole of my projected journey, I started from Naples, and proceeded by the Porta Capuana along the great road made in the reign of Charles V. through Apulia, which being the only one existing in the whole country, excepting that to Rome, excited great admiration at the time, and is celebrated by elaborate Latin inscriptions placed upon blocks of stone, that still stand on the road's side, along a considerable part of the line, at intervals of only a few miles.

The first portion of the way lay along a level plain, which in the vicinity of Naples is marshy and unwholesome, from the overflowings of the waters of the Sebeto, once a considerable stream, but now, from the changes effected in the level of the country by the eruptions of Vesuvius, contracted to an insignificant rivulet, which loses itself chiefly in swamps, without reaching the sea. As we proceeded, the country on either side appeared to increase in fertility; and though we were in the midst of winter, yet it was easy to imagine the luxuriance which it must present in autumn, when the elms and poplars, which extend along the whole of this great plain to the very foot of the Appennines, continue in full verdure, and are united by festoons of vines, stretching from tree to tree, and loaded with a profusion of fruit.

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[a] For the distances, see the post road given in the Appendix.

The substratum is made up of a volcanic tuff, such as we see every where in Campania, the wide extent of which plainly indicates, that volcanic action had been rife in the country, long before Vesuvius began to emit lava, and when the country was still under water.

Near the village of Cisterna, a few miles from Naples, I perceived a quarry of leucitic lava, very similar to that of the Monte Somma, and evidently constituting a stream which had descended from that mountain, and not from Vesuvius. Since indeed the focus of eruption has been transferred to the present crater, the whole of the northern skirts of the mountain have been screened from lava-currents by the intervention of the M. Somma, the summit of which constitutes a great semicircular wall, having betwixt it and the crater of Vesuvius, the valley known by the name of the Atrio del Cavallo. The existence of this lava-current would therefore in itself furnish a sufficient proof, were any wanting, that antecedently to the famous eruption in the reign of Titus, to which we are led to attribute the commencement of the eruptions of Vesuvius itself, and the breaking away of the great crater, of which Monte Somma still forms a part, lava-currents had been given off from that part of the volcano, which is now in a quiescent state.

Having nearly reached the foot of the Appennines, we turned off from the direct road to visit Nola, a town of much classical repute, but at present presenting scarcely any remnants of antiquity. Those who, like myself, are induced to diverge from their route in order to visit it, will do well to inspect a collection of Etruscan vases, belonging to a priest resident in the place, which, though not large, appears extremely choice.

After leaving Nola, we soon began to ascend the Appennines, a mass of limestone for the most part destitute of petrifactions, though generally regarded as coeval with that of the Jura mountains; which latter again appear to have been identified in point of age with the oolitic rocks of England.

This range of mountains stretches like a great wall from the north-west to the south-east, extending from the Alps to the very foot of Italy, but leaving on either side of it a considerable tract of comparatively level ground, of which the Campagna di Roma, the Pontine marshes, and the Terra di Lavoro round Naples on the west, and the flats of Taranto, Bari, and Otranto on the east, may be cited as examples.

That the tuff which we found on the plain should also extend to a considerable distance up the slopes of the Appennines, I was fully prepared to expect from having observed the height which the same stratum attains near Naples, at the Camalduli, and in other places.

But it is interesting to be able to trace proofs, in the relative position of the Appennine limestone and the volcanic tuff, of the elevation of the former rock, and even its excavation into valleys, having taken place, prior to the formation of the volcanic matter which rests upon its flanks. This I infer, from the appearance of the limestone jutting out, and, as it were, protruding itself in several places, at an elevation much inferior to that which the tuff is seen to attain; and likewise from the occurrence, high up on the chain, of lateral valleys, the summits of which are capped with limestone, though the hollows and sides present only tuff. The sides of the road which we ascended afforded us an excellent section of the materials of which this volcanic deposit is composed. I found it to consist of an alternation of beds, of volcanic tuff, and of pumice of various colours and consistence. The great thickness which the mass must have attained, when it extended over the plain of Campania, as it once appears to have done, to a considerable height above its actual level, and the vast number of strata which might be counted on our ascent, are circumstances which forcibly arrest the attention of the scientific traveller, but which more properly belong to the general history of volcanos, than to the present detail of local phenomena[a]. I may however remark, that this filling up of valleys previously existing in the older rocks of the country, by volcanic tuff, is by no means peculiar to this locality; it is observable at Sorrento on the one side of Naples, and at Caserta on the other; but nowhere perhaps is it so well seen as on the road into Apulia, both from the many miles over which it there extends, and from the section constantly exposed by the great military road which intersects it.

Having reached the summit of the Appennines, we were rewarded by a magnificent view on either side: on that of Naples, the Terra di Lavoro, unrivalled for fertility, lay stretched before us in all its beauty; but still more striking was the prospect on the side of the Appennines.

On a hill to the right is the picturesque ruin of an

[a] See Appendix.

ancient fortress, called Monte Forte, very formidable in the middle ages, and also celebrated, if I was rightly informed, as the spot where the last ill-fated Neapolitan revolution first broke out.

On a much loftier hill to the left, called Monte Vergine, the sides of which are clothed with chestnut trees, formerly was the site of a temple of Cybele; now, without any diminution of its former sanctity, dedicated to the Madonna, to whose honour a monastery has been erected on the summit.

Once a year, thousands from all parts, regardless of the steepness of the ascent, toil up the mountain, either to offer their adorations at the shrine, or under the influence of the various human motives which conspire to swell the numbers collected on such occasions.

Lastly, in front of us was stretched the elevated table land of the province of Principato Ultra, which, although diversified every where by valleys and acclivities, is throughout raised considerably above the level of the great plain of Campania.

Accordingly our descent was much less considerable than our previous ascent had been; and after reaching the little village which lies under the old fortress of Monte Forte, an avenue of poplars, nearly a mile in length, brought us to the town of Avellino.

Avellino itself is a large and populous place, the capital of the province, and the residence of the governor or intendant. It is now, as of old, famous for its orchards and its hazel-nuts. Virgil celebrates the former,

> Et quos maliferæ despectant mœnia Abellæ;

and the name of nux Abellina given to the hazel-nut indicates the abundance of that tree in its neighbourhood. To the stranger, however, it presents few objects of interest; and all I need remark further respecting it is, that the traveller will meet there with a very tolerable inn, and that he will find it convenient to make it his starting-place for the Lake Amsanctus, which may be reached from it on horseback in little more than half a day, thus allowing time, in the fine season, to proceed onwards before night-fall to Ariano, where decent accommodations may likewise be secured.

Being encumbered with a carriage, I was compelled to take a more circuitous route, which traversed an upland, and, for the first part of the way, an highly picturesque country, passing through Pratola and Mirabella, where I diverged from the main road, in order to reach the little town of Frigento, situated about five miles to the east of it.

For some miles after leaving Avellino, I was surprised at observing a continuation of the same strata of volcanic tuff which I had traced all the way during my ascent; so that, as it is quite evident that no volcano exists in the neighbourhood, from which they could be derived, we can only suppose that the waters which washed this material into the Appennine valleys, as before noticed, extended to such a height as to spread it over the crest of the mountain chain which we crossed on reaching Avellino. All traces of volcanic matter appeared however to have ceased several miles before we turned off from the main road at Mirabella, and nothing appeared but the blue compact limestone of the Appennines, alternating with beds of grit, but uncovered by any other material.

Frigento itself, like most of the towns in Apulia, is built on the brow of a hill, and is inaccessible for carriages: leaving ours, however, at an osteria below, we ascended it on foot, and found there an hospitable reception in the house of Don Martino, one of the principal proprietors of the place, who had been prepared for our visit by a letter from a relation at Naples, and was looking out for our arrival.

Nothing, I am sure, could exceed the hospitality and kindness we experienced during our stay from this gentleman and his brother the Canonico; but for the sake of those who may follow a similar route, I will remark, that in this bleak and exposed country, December is not the month to enjoy comfort in the country houses of the Neapolitan gentry. In all parts of the world the habits and fashions of the capital are more or less a law to the provinces; and as the Londoner has introduced at Naples the grates and coal fires which he uses at home, so in the little town of Frigento, the very name of which (Frigento, a frigore) indicates its bleakness, the casino of my friend Don Martino was built after the fashion of those in the plain of Campania, and boasted not a single fire-place in any one apartment excepting the kitchen.

There accordingly the whole family (for they were by no means Stoics as to cold) crowded after their evening repast was concluded, and being joined by a number of their neighbours, who had been attracted by the uncommon sight of an English traveller, formed a considerable circle round the hearth.

When we retired from this room, a brazier was the only mode of obtaining warmth that remained for us; and it is one which, except in such severe weather, we should hardly have wished to resort to, from the oppressive and unwholesome fumes which attend its use.

The next morning I proceeded to the lake or pool of Amsanctus, which is situated in the plain below, about midway between Frigento and another similarly situated town called Grotta Maina [b].

The lake and valley of Amsanctus is familiar to every schoolboy, as the spot through which Virgil represents the fury Alecto, after having excited discord between Turnus and Eneas, as descending to the infernal regions.

Some indeed have suggested, that Virgil alluded to certain sulphureous ponds that occur near Rieti, above the famous falls of Terni, where the immense accumulation of travertine proves, that the extrication of carbonic acid, and probably of other gases usually accompanying it, has been taking place on an enormous scale; but the locality seems to be fixed by a passage in Cicero, who expressly says, that the Lake Amsanctus lies in the country of the Hirpini, "Quid enim? non videmus quam sint varia terrarum genera? Ex quibus, et mortifera quædam pars est, ut et Amsancti in Hirpinis, et in Asia Plutonia." A passage in Pliny might also be quoted to the same purpose, but the description which Virgil gives is so applicable to the spot, that I conceive no doubt can remain as to its identification. Thus Ansanto stands "Italiæ medio," (midway between the Adriatic and Mediterranean,) "sub montibus altis," (having in its immediate neighbourhood the hills of Frigento, Villa Maina, &c., and at a still greater distance, the mountainous heights of Monte

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[b] Mr. Hamilton has published in the third volume of the Geographical Journal an account of his visit to this spot.

Vergine, and other of the loftiest points in the chain.) It stands in a little valley, which till lately was flanked on either side by a thick wood,

> densis hunc frondibus atrum,
> Urget utrinque latus nemoris,

for it was only in the time of Murat that the wood on the right hand was cut down; whilst a small mountain torrent flows down the valley, and supplies the lake,

> medioque fragosus
> Dat sonitum saxis, et torto vortice torrens.

Here from the pool, which occupies a particular spot where the valley is somewhat wider than elsewhere, issues an enormous quantity of noxious gas, whilst the same proceeds copiously from a cavity in an adjacent rock, so that the latter part of the poet's description is exactly verified,

> Hic specus horrendum, sævi spiracula Ditis,
> Monstratur; ruptoque ingens Acheronte vorago
> Pestiferas aperit fauces.

Lastly, they point out on an adjoining eminence some vestiges of the neighbouring temple to the goddess Mephitis; and Romanelli notices an inscription found on the road from Ariano to Montecalvo, which makes mention of a votive offering presented to the goddess by a Roman lady,

> Paccia Quintilla Mefiti votum solvit.

In our visit to this place, after we had descended the hill on which Frigento stands, we shortly came upon a small pool of water which, like the larger one to which attention has chiefly been directed, emits a quantity of gas. This I collected in the usual manner, by inverting a jar full of water over the spot, on the surface of the pool, from which the bubbles issued; and on my return to Frigento in the evening, submitted it to chemical examination, and found it to consist partly of carbonic acid, and partly of sulphuretted hydrogen. After the removal of these two gases, however, there was a small residuary quantity of air, which contained about 16 per cent. of oxygen and 84 of nitrogen.

We next reached the narrow valley, or rather watercourse, in which the larger pool of Amsanctus is situated. Nearly two miles below it is a warm spring, which disengages carbonic acid and sulphuretted hydrogen gases, and which in the lapse of ages has produced on either side of the water a thick deposit of travertine.

A little beyond us, towards the south, was the town of Villa Maina, standing on an eminence, which I am induced to notice from a circumstance, stated to me by a resident gentleman, who was of the party, respecting it, which seems worthy of notice.

He assured me, that the health of the inhabitants had appeared to suffer materially, since the cutting down of the wood which lay betwixt them and the mephitic lake; that they were sensible of the odour proceeding from it when the wind blew from that quarter, which was not the case before; and that they attributed the insalubrity of the situation to that circumstance. My informant further assured me, that the inhabitants of Villa Maina are noted for the sallowness of their complexions, and that the disease to which they are chiefly subject is an affection of the liver.

Fanciful as it may appear to connect any disorder of the animal functions with the minute quantity of a deleterious gas, which can be so wafted to a distance, so great as four miles from the place of its emission, I am not disposed altogether to pronounce it impossible; for without being a convert to the doctrine of homœopathy in the extent to which it is carried, I can conceive, that the long continued action of a very minute quantity of a noxious ingredient may sensibly influence the system, whilst an infinitely larger dose, applied during a short space of time, may be thrown off without inconvenience, in consequence of the resistance which the vital principle opposes to its introduction, or, as our predecessors in the medical art preferred to denominate it, the *vis medicatrix naturæ.* Thus it is at least not absurd to suppose, that an individual who had exposed himself for some hours to these vapours within the crater of Vesuvius, in the most concentrated state in which they can be endured without subsequent injury, might be affected by inhaling, during many years, a very minute portion of the same, wafted from the Lake Amsanctus, whenever the winds set in that direction.

The quantity of mephitic vapour which proceeded from that quarter was such, as to oblige us (the wind being in the north) to take a circuit towards the east, in order not to meet the noxious blast; instances not unfrequently occurring of animals, and even men, who have imprudently ascended the ravine, being suffocated by a sudden gust of air wafted from the lake.

This is the origin of the fable of the Vado Mortale, a particular spot in the course of the rivulet that flows from the lake, which, it is said, cannot be forded without death, and which has been described, as having on its borders an accumulation of the whitened bones of the various animals that had perished there, which must for ever lie on the spot where they fell, because no living creature can approach to take them away without adding to their number.

The wonders of the Lake Amsanctus are sufficiently striking without such exaggeration; no bones existed in the valley at the time I visited it, excepting of some birds, who, in crossing the valley, had been arrested on the wing by the noxious effluvia, as at the Lake Avernus of old; neither even close to the lake, where the evolution of gas is most abundant, is there any point at all times unapproachable, for we ourselves were able to reach its edge on the side from whence the wind blew.

I have already stated, that the pool itself stands in a part of the ravine, which is widened out considerably, contracting again to its original dimensions below. It has been stated as being about 20 paces in its smallest diameter, and 30 in its largest, which I conceive to be near the truth; and its depth is said to vary from 6 to 7 feet. From the quantity of gas which is continually escaping, it appears to be throughout in a state of violent ebullition, but its temperature little, if at all, exceeded that of the surrounding atmosphere.

The colour of the water is dark and muddy, from the quantity of sediment projected towards the surface, owing to the constant agitation into which the pool is thrown by the gas that rises up through it; its taste strongly bespeaks the presence of alum, which is said to render it efficacious in the cure of certain diseases of cattle.

One of the guides, who approached its edge, filled a bottle with the water; but to have collected the gas itself, would have been a perilous attempt.

I can only infer therefore, that it resembles that which issued in smaller quantity from a more inconsiderable pool within a hundred yards of the spot, and which consisted mainly of carbonic acid gas. The smell, however, plainly indicated that sulphuretted hydrogen was likewise emitted at the former vent; and the consequences of the long-continued action of this gas upon the constituents of the contiguous rock, was not one of the least interesting or instructive parts of the phenomena presented in this locality.

The rock itself, every where in the vicinity of the Lago d' Ansanto, is the common blue fissile limestone, interstratified with beds of grit, which had accompanied us from Avellino. There is nothing that reminds us of volcanic action, except about the lake, where those products, which usually are found in solfataras, and in other spots exposed to the action of sulphureous vapours, abound.

Thus the rock immediately round the pool consists of a pulverulent earthy substance, derived from the conversion of the carbonate into sulphate of lime, and the production of alum from the union of the same acid with alumina; sulphur, derived from the sulphuretted hydrogen gas, also impregnates it in sufficient quantity to be worth collecting; and in some places the colour and smell indicate the presence of petroleum, disengaged along with the sulphur from the depths below.

Moreover, as a proof that this action is not altogether local, and that it is capable of producing effects on a scale commensurate to those exhibited in several of our rock formations, we observe at a short distance, resting upon the Appennine limestone, a bed of gypsum nowise different from those occurring in many tertiary deposits.

Now it was interesting to me to remark the analogy between the appearances presented here, and those at the Lagoni near Volterra in Tuscany, which I had visited on my way to Naples just before. There, too, volcanic rocks are entirely absent; and the formation for many miles round the spot consists of the Appennine limestone, with serpentine or diallage rock protruding through it.

But at the Lagoni we see pools of water in a state of absolute ebullition, from the quantity of steam which is constantly rising through them, and which imparts to them a temperature exceeding 180° of Fahrenheit. This steam seems to carry up with it boracic acid and sal ammoniac; the former in sufficient quantities to make it worth while to evaporate the water through which it passes in order to collect it; the liquid being conducted into shallow troughs, where it is mixed with soda, by which addition crystals of borax are obtained from it as the aqueous portion escapes.

Now when we compare together the effects produced by the disengagement of steam and sulphuretted hydrogen, owing almost unquestionably to a volcanic cause, in the instance before us and in that of the Lake Amsanctus, we are naturally led to apply the same explanation to those immense deposits of sulphur which occur on the western side of Sicily. If any doubt should exist as to the fact of their having been so produced, it may be removed by reflecting, that we know of no instance of sulphur being sublimed in an uncombined form by volcanic action, and that it seems scarcely possible for such an event to occur, without the combustion of the sulphur taking place the instant of its coming into contact with atmospheric air.

Hence it may be inferred, that the whole of this vast deposit in Sicily has been occasioned by a decomposition of sulphuretted hydrogen gas, such as has taken place on a smaller scale at Lake Amsanctus, and has there impregnated the surrounding rocks with crystals of the same material.

In Sicily too we meet with all the combinations which sulphuric acid is capable of forming with the earths present—in the beds of sulphate of lime, of strontian, of barytes, of magnesia, that occur—there also we see in the immediate neighbourhood warm springs impregnated with sulphuretted hydrogen—memorials, as it were, of the cause, which had produced these deposits.

May we not also be led to conjecture, that the gypsum so commonly present in the tertiary clay of Volterra and the maremnæ of Tuscany, has been produced by the same process, especially when we find this clay associated, as it frequently is, with beds or nests of sulphur.

Thus the formation of the blue clay, in Sicily and in the maremnæ of Tuscany, would have taken place, not only at the same epoch, which is generally admitted to have been the case, but under the same physical conditions, one as the other, and a volcanic action similar to that going on at the Lago d' Ansanto and at Monte Cerboli would likewise have given rise to the deposits, which the former contain, in common with the rocks found immediately round the spots, where the above operations are at present proceeding.

But there is another circumstance also worth noticing, although the inference to which it seems to point will scarcely receive admission until further proof can be adduced in support of it. I allude to the association of salt springs with gypsum and sulphur both in Tuscany and Sicily. Their occurrence in such localities as these might induce us to conjecture, that the same volcanic action, which produced the sulphuric salts, and volatilized the sulphur, has been instrumental also in separating the salt from its solution in water, and thus serve to explain, in these instances at least, the puzzling fact, that rock-salt is found associated, as is so commonly the case, with beds of gypsum. The connexion between the above phenomena may perhaps be seen more clearly by the following tabular view:

Volcanos give out	sulphuretted hydrogen, sal ammoniac, boracic acid, muriatic acid, steam;
and cause	deposits of sulphur, of sulphuric salts, of muriatic salts, &c.
Moffettes, connected geographically with volcanos either now in action, or extinct, give out	the same principles,
and cause	deposits of sulphur and of sulphuric salts.
Many tertiary clays, some of which are connected geographically with volcanos, contain	beds of sulphur, of earthy sulphates, and of common salt.
Most salt formations are associated with	beds of gypsum,
some with	sulphur,
others with	sal ammoniac.

To return from this digression to the Lake Amsanctus.

It is interesting to remark, that the position of this spot is almost exactly intermediate between the active volcano of Vesuvius and the extinct one of Mount Vultur, and that a straight line drawn from the one to the other, and which might likewise be extended on to the volcanic island of Ischia, would pass within a mile or two of this mephitic lake.

From the neighbouring eminence on which Frigento is situated, the summits of both mountains were alike visible; and it was stated to me on the spot, that when Vesuvius was in a state of activity, an unusual quantity of gas was disengaged from Amsanctus. Hence it seems probable, that the same elements of volcanic activity exist underneath the earth all across the peninsula, although these elements may be called into more intense action on either side of the Appennine chain, by the proximity of sea water, or by some other circumstances which do not occur in the central portion of the chain.

The occurrence of Plutonia, or places which disengage large volumes of noxious gas, is familiar in all volcanic countries, but I know of no other instance, in which the quantity is considerable enough to endanger life in the open air round about the point of its emission, except that of the Valley of Death in Java[a].

[a] It is a valley near the summit of a mountain, about half a mile in circumference, the depth from 30 to 35 feet, the bottom quite flat, no vegetation, a few large (in appearance) river stones, and the whole covered with the skeletons of human beings, tigers, pigs, deer, peacocks, and a great variety of birds and beasts. The party who visited it could not perceive any vapour or

The accounts given of the dangers of this valley, combined, by an extraordinary confusion of ideas, with those respecting the Upas tree of the same island, which was said, like the Manchineel of the West Indies, to be noxious to those who slept under it, gave birth to the monstrous eastern fable concerning the tree reported by Darwin;—of its tainting the air around for several miles by its pestilential effluvia; of its converting the whole neighbouring country into a desert by its malignant influence upon vegetable, as well as animal life; and of its yielding a poison, so subtle as to destroy life by a single drop, and so dangerous to those who collected it, that the malefactors who undertook that service for the emperor, received a free pardon, if they returned alive [a].

These two localities possess an interest independent of the romance which attaches to their history, as shewing that causes are in operation at the present day in spots remote from ordinary volcanic phenomena, on such a scale, as would in a comparatively short period modify the character of the earth's surface over a considerable surface, and produce new deposits bearing a comparison in kind and in extent to those which geological observation presents to us.

The following morning I left the hospitable roof

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opening in the ground, which appeared to be of a hard sandy substance. The sides of the valley, from the top to the bottom, were covered with vegetation, trees, shrubs, &c. They descended within 18 feet of the bottom, below which the air was too noxious. Dogs and fowls sent in to a lower point died in a few seconds. The human skeletons are supposed to have been rebels, who had been pursued from the main road, and had taken refuge in these dangerous valleys. There is no smell of sulphur, though the whole range of hills bordering on the spot is volcanic.

of signor Martino, and regaining the great road at Mirabella, proceeded onwards towards Mount Vultur. The first town we reached on our road was Ariano[b], situated on the summit of an eminence, composed, not of the Appennine limestone which we had hitherto met with, but of alternations of loose sand-stone and marl, resting upon a conglomerate full of pebbles, chiefly composed of this older calcareous rock. The sand-stone abounded in shells, but they were extremely fragile; murex, turbo, buccinum, venericardia, ostrea, and anomia, were the most common genera.

In order to explain the occurrence of these tertiary marine deposits at such a height on the centre of the Appennines, I cannot do better than quote the words of Brocchi[c].

"At one period the habitable part of Italy was reduced to the ridge of the Appennines, which formed a long and narrow peninsula connected with one of the heights of the Alps. Though we must not suppose this tract to have been quite continuous, yet I believe that it extended over a great space without a break, since the valleys which intersect the Appennines, supposing that such as now exist were there at the time, are neither very numerous, nor sufficiently deep, to have connected the two seas together, and to have converted the ridge of the Appennines into a succession of islands. In fact the principal passes which traverse this chain of mountains, are carried over eminences of considerable height, such as that of Abetoni between Pistoya and Modena, which has an elevation, according to Pini, of 4166 feet; that of Pietramala between Florence and Bologna, Furlo near Fossombrone, Colle Fiorito between Terni and Foligno, Bocchetta in Liguria. In order to determine with precision the length of this ancient peninsula, and to mark the spots where its continuity was broken, we ought to ascertain what was in different places the elevation and depression of the central portion of the Appennines, previously to the corrosions and rents which the rivers and torrents had in after-times effected. One of these interruptions certainly existed in that part of the kingdom of Naples, which bears the name of the Principato Ultra, and was in a valley which is prolonged from Avellino to Bovino, and which leads from the Terra di Lavoro into Apulia. This valley at one time cut through those Appennines to which belong the gigantic calcareous mountains of Incoronata, Monte Vergine, and Avella, for if on the side of Ariano, or in its central portion, it is seen to be closed in by great eminences, the latter have been formed by deposits analogous to those of the hills of Tuscany and the Romagna, consisting in a calcareous sand with grains of silex and scales of mica, which at Monte Reale, between Bovino and Ariano, as likewise between this latter place and Grotta Minarda, is seen to be superimposed on a marly bluish clay. This sand extends from the side of Apulia nearly to Ordona, and is prolonged beyond Naples, though near Pratola it begins to be covered with a volcanic tuff derived from the substances erupted from the submarine volcanos of Campania, or transported by the waters which insinuated themselves into the hollows and recesses of the surrounding mountains.

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[b] Supposed by some to be the Equus Tuticus, referred to by Horace, as the "oppidulum quod versu dicere non est." Its situation on the top of a hill might account for the scanty supply and dearness of water:

"venit vilissima rerum
Hic aqua."

But Dr. Cramer places the old town alluded to five miles further north, where the church of St. Eleuterio now stands.

[c] Concholog. subappenn. page 58.

" The valley of Bovino then separated from the rest that tract of country comprehended under the Basilicata, Apulia, and Calabria, which then appeared like a distinct island, or rather was an union of islands formed by the highest peaks of these mountains, since the Murgiæ of Apulia, and the low eminences, which, branching through the territory of Otranto, extend as far as Cape Leuca, then continued submerged."

On this description of Brocchi's I would only observe, that the whole of the valley spoken of must not be inferred to be covered with tertiary deposits: on the contrary, the line of road I pursued from Avellino to Frigento, and from Frigento to Ariano, presented only the Appennine limestone with its subordinate beds, until we came within a few miles of the latter town, which stands on a lofty eminence, composed entirely of tertiary beds, resting on a loose conglomerate.

The softness of the stone of which the rock consists has rendered it so easy to hollow out, that the whole surface towards the south is honeycombed into caves, which form the dwelling-places of a large proportion of the population, as is the case in one or two places in the tertiary deposits of central France, at Lisle, &c. It is remarkable that the people, and more especially the children, who emerged from these damp and sombre habitations, seemed perfectly healthy, contrary to what has been observed in some other places of the kind, where, in consequence, as is supposed, of the absence of light, the form and functions of the animal system are but imperfectly developed[c].

From Ariano we proceeded along the valley till about nightfall, when we reached the bridge of Bovino, the town itself standing on an eminence above.

This spot, I may remark, was formerly infamous for the robberies committed in it; the locality being peculiarly favourable for such enterprises. For the last few miles the road lay in a narrow glen, hemmed in on either side by a steep ridge, covered on the top with wood, from whence the robber could watch his prey, and plan his attack at leisure. Add to which, that there were no means of escape, except through the pass at which the bridge of Bovino stands, and that for several miles before reaching it, there is no track to the right or left branching out of the main road, by which the traveller might avoid the danger awaiting him.

Even now, that the bands which formerly infested the country are destroyed, videttes continue to be planted at distances of a mile or two along the whole of the pass, and few choose to delay their journey through it till the close of day.

Finding only a most filthy inn at the bridge of Bovino, and not relishing the idea of being benighted in such a country, we were fain to put up at an *osteria* a few miles beyond, where we should have fared but badly, had we not brought with us our own provisions, and been able to avail ourselves of our carriage as a retreat for the night.

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[c] See Dr. Edwards' experiments on tadpoles, which, when secluded from light, never underwent their metamorphosis into frogs, though they continued to increase in size.

At the bridge of Bovino we took our leave of the Appennine chain, and found that we had begun to enter upon the wide plain, which extends to the shores of the Adriatic, a distance of nearly thirty miles. This tract seems to be wholly composed of very recent deposits, and to have constituted at a comparatively modern epoch, a gulf of the sea, which was at that period circumscribed, on the south-west by the range of hills which stretches from Bovino to the neighbourhood of Melfi, and is from thence prolonged to the present gulf of Taranto, and on the north by the heights of Mount Gargarus in Manfredonia; both equally belonging to the Appennine chain, and consisting of older formations.

At that point, we quitted the great road into Apulia, which had brought us thus far with ease and safety, and had to plough our way as best we could, through a wild uninclosed tract of land, chiefly devoted to pasturage, but, so far as relates to roads, and other evidences of human cultivation, in a condition little more advanced than when it first emerged from the ocean.

To those who are disposed to follow my footsteps, I may suggest, that this part of the journey would be accomplished far more expeditiously, as well as more agreeably, on horseback, in which case the traveller might, if well-mounted, reach Melfi, the nearest town to Mount Vultur, in one day from Bovino, by setting out at an early hour.

Encumbered by a carriage, and travelling at a season, when the sandy loam over which we had to pass, was rendered stiff and heavy by the rains that had fallen, we could get no further that day than Ascoli, a wretched town, where we procured very sorry lodgings at a little inn kept by an old woman, possessing a strongly marked physiognomy, but whose haggard looks and wild expression of countenance might have suited one of the witches in Macbeth. Being curious to learn her history, I made inquiries of a person of the town with whom I chanced to converse, and was told, that she was the sister of one of the famous robber-chiefs of Apulia, and in her youth had possessed considerable personal charms; that she had lived with her brother and his troop during many years in their mountain fastnesses, and had shared their prosperous days; but when the band was broken up, and her brother had been apprehended and executed, had found it necessary to take up with the occupation of an innkeeper, which in my informant's opinion was a great sinking of caste. His feeling on this last point will seem more intelligible, when we recollect, that at a time not very remote, the leaders of the banditti in Apulia were generally persons of some rank; that they often combined a wild feeling of chivalrous honour with their lawless habits of life; and that whilst their power made them objects of terror to all, their system of confining their outrages to the rich, created a kind of fellow-feeling in their behalf amongst the humbler classes.

Ascoli, the ancient Asculum, is celebrated for the second battle between Pyrrhus and the Romans, which, though hotly contested, terminated without any signal advantage on either side. It stands on a hill, based on a shining, glary, sandy clay, upon which rest beds of yellow sand; the whole being capped with thick beds of conglomerate, in which rolled pebbles of the Appennine limestone, and of some other rocks, none of which however are volcanic, are firmly agglutinated together by sandstone, so as in some places to constitute a very coherent mass. I could perceive no organic remains in any part, but have no doubt, from its general aspect, that it is of tertiary origin.

The next morning we prosecuted our route to Melfi, passing at first over a plain as wild and as uncultivated as that we had traversed on the day preceding. About nine miles from Ascoli we crossed the Ofanto, the Aufidus of the ancients, a river at the time we passed it of very inconsiderable size. We had next to ascend several pretty lofty ranges of hills, from the summit of the last of which the town of Melfi was visible.

From this point the view that opened upon us was singular and beautiful; at the extremity of the landscape stood the Mount Vultur, which in its general outline bears a striking resemblance to Vesuvius, as we imagine it must have been before it broke out in A. D. 79.

Between the spot where we were and the mountain, stood the town of Melfi, built upon an isolated neck of land, with its castle, a picturesque building, overhanging the precipice. A little stream winds round the valley, dividing us from the rock on which Melfi is situated, and a bridge thrown over the stream at the bottom of the valley connects the town with the road. No volcanic appearances presented themselves until we reached the range of hills nearest to the former, previously to which, the tertiary sand and clay of the neighbourhood of Ascoli, covered with puddingstone, containing pebbles of the Appennine limestone, alone made its appearance.

But the range immediately south of Melfi seemed to be based upon the older Appennine limestone, covered over however by beds of tertiary sandstone capped by volcanic tuff, so that, as we descended the valley intervening between this eminence and the town of Melfi, we soon found ourselves travelling upon that limestone.

The hill, however, upon which the town itself is built, consists of different materials. It is based upon beds of volcanic tuff, which (as may be seen in the annexed drawing) dip from the Mount Vultur, so that at the north western side of the rock of Melfi they lose themselves under the surface. These are covered by a thick bed of lava, which presents a precipitous escarpment, as well as an imperfect columnar structure, towards the north-west, and appears to have proceeded from Mount Vultur, though the changes which the country has since undergone may have severed the connexion with its parent.

At Melfi I found tolerably comfortable accommodations at the Nobile Locanda di Sole, and every possible attention from the gentlemen resident there to whom I had brought letters.

The town appears not to have existed in the times of the Romans, but to have been a place of some repute in the middle ages, when the Normans, under Count Ranulph, seized and fortified it. The investiture of the Dukes of Apulia accordingly took place there, and it was at that time considered one of the principal cities of the province.

It is at present a large but an extremely dirty town, principally belonging to the prince of Doria.

It lies so much out of the general route, that the intercourse with foreigners is very small; and the people, though honest and obliging, are ignorant and superstitious to a degree which the stranger will do well to be aware of.

Almost the last scientific traveller before myself, who made Mount Vultur the object of his researches, was the celebrated Italian geologist, Brocchi, who, burdened, not only with a hammer, but likewise with a barometer and other philosophical instruments, proceeded some fifteen years ago to ascend the mountain.

The peasants, at a loss to conjecture the nature of his objects and his manipulations, set him down as a magician, but not knowing whether he might have come to do them good or harm, contented themselves at first with watching him, very attentively, but at a respectful distance. Unfortunately, in the very midst of his observations, the heavens lowered, the wind betokened an impending tempest, and drops of rain began to descend.

The peasants regarded all this as the first-fruits of his incantations, and awaited with silent dismay the result; but when they saw a furious thunder-storm invading their crops, and demolishing their hopes of a coming vintage; whilst the philosopher continued inspecting with redoubled attention those instruments of his craft, which had been regarded by them before with so much suspicion, fear gave way to indignation, and they rushed forwards in a body with the full intent of demolishing the mysterious author of all this mischief.

Fortunately for Brocchi, he had with him three or four resolute gens d'armes, as his escort; and these, with their muskets and bayonets, contrived to keep the unarmed peasants at bay long enough to enable him to escape, or his zeal in exploring the secrets of volcanic action might have been as fatal to him, as it was of old to Empedocles[f].

The following morning I ascended mount Vultur in company with a gentleman of Melfi, who insisted on being of the party, and two gens d'armes who were sent as an escort, in addition to the ordinary guides, &c. We first wound round the western flank of the mountain, keeping on our right hand the valley, at the bottom of which flows the river Ofanto. Wherever the substratum was exposed, it seemed to consist of volcanic tuff, of which various beds were visible, some very compact, others loose and friable, consisting chiefly of pumice like those about Pompeii[g].

In this loose material we saw several caverns excavated by art, some of which, we were told, had been the lurking-places of those bands of robbers, who within the last twenty years laid the country under contribution, and made mount Vultur their principal retreat.

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[f] According to a recent tourist in Ireland, a similar adventure lately befell a botanist herborizing in the mountains of Connemara during the time the cholera was raging.

He was nearly murdered by the country people, in consequence of being mistaken for a French doctor sent by government to inoculate the people with this dreaded plague; and the tin box, in which he carried his plants, was regarded by them as the case, in which his stock of infection was kept, in readiness to disseminate wherever he went. See Angler in Ireland, vol. i. p. 189.

[g] The neighbourhood is said to yield a profusion of plants, and being almost unexplored by modern botanists, is likely, if visited at the proper season, to yield a rich harvest to the collector. Like most volcanic soils, it produces excellent wine.

After skirting the base of the mountain in this manner, we came at length to the wood, on its southern flank, termed Montecchio, and, having crossed a deep ravine, in which flows a ferruginous water, tinging the stones over which it passes with a red ochreous sediment, entered an extensive forest, well calculated to be the haunt of the numerous banditti who formerly resorted there.

In the midst of this forest is a spring called the Aqua Santa, quite cold, and without any particular taste, but possessing some reputation in the cure of diverse disorders, and giving out bubbles of air. This I collected, and ascertained to consist of carbonic acid gas, together with a small portion of residuary air, amounting to about 1 p^{r}. c^{t}., 100 parts of which contained 10 of oxygen, and 90 of nitrogen[h]. The same gas escapes in still larger quantities from a fissure in the rock hard by, and a considerable deposit of travertine in the neighbourhood indicates, that it has been given off elsewhere[i].

From this point we began to ascend in a northerly direction, and upon reaching a particular point about half way up the mountain, found ourselves in the midst of an amphitheatre of hills, which from its form, as well as the physical constitution of the rocks surrounding it, immediately suggested to us the idea of its having been at one time the crater of the volcano. The cavity was nearly circular, and the brim, with the exception of the side by which we had ascended, was almost entire.

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[h] Thus confirming the inference which I have deduced from many former observations, that whenever atmospheric air escapes from a volcanic source, it is found deprived of a portion of its oxygen.

[i] The same gas is emitted at a place called Rendina, to the east of mount Vultur, and likewise, together with sulphuretted hydrogen, from some mineral waters near Atella, on the western side of the mountain.

Its interior was covered with a rich black soil, having but few blocks of volcanic matter scattered over its surface. The rocks encircling it are very irregular in point of height, some of them rising more than 1000 feet above the average level of the margin. One of these eminences, which partakes of a conical form, constitutes the summit of the mountain; for, as is the case in some other instances, there is no crater on the highest point.

Within this great circular expansion, are two minor depressions, which are likewise nearly round, and, as they occupy the lowest portions of the crater, receive the drainings from other parts. Hence they form two lakes communicating by a narrow outlet one with the other, and discharge their superfluous waters by a little rivulet which runs from the lower or more southern. The water of the latter is perfectly sweet, that of the most northern one is stated to be salt, but it must be slightly so, since fish abound in it.

It is said, that at times volumes of sulphuretted hydrogen, or some other inflammable gas, are given off from this lower lake; and jets of water have been known to be suddenly thrown to the height of fifteen feet above its surface. These circumstances evince, that the volcanic action is not completely spent, and still shews itself at intervals, in an energetic manner, by the evolution of gas. On the borders of the upper lake are the ruins of a church, dedicated to S. Hippolito, and on an eminence overhanging it is a convent of Franciscans, who, protected by the sacredness of their calling, and their supposed poverty, continued to reside without molestation in this sequestered spot, at a time when the mountain itself was the head quarters of the bands of robbers which infested the province. Once a year Melfi pours forth its whole population on a pilgrimage to this sacred spot—prompted by the double motive of paying their adorations at the shrine, and of commemorating the escape which a portion of the inhabitants experienced in 1528, by secreting themselves within the crater, and thus eluding the fury of the French troops under Lautrec, who took the town by assault, and put all that remained in it to the sword[k].

The rocks, which encircle this crater, seem generally to consist of tuff, but the substratum is so completely covered with vegetable soil, that it is difficult to determine its stratification. One side of it is covered with a thick forest inhabited by wild boar, the rest is cultivated, and yields abundant crops.

A heavy shower of sleet obliged me, to abandon my original design of ascending to the summit of the mountain, and to hasten homewards.

We accordingly climbed the western edge of the crater at its lowest point, and having reached the summit, descended the opposite side in the direction of Melfi. Not a single stream of lava was observable on the flanks of the mountain on this side; if any such there be, they were concealed by a thick bed of black unctuous soil, resulting from the mixture of volcanic ashes with the clay which proceeded from the decomposition of harder materials. The extent to which this has taken place,—the rare occurrence, in a mountain which once was the theatre of such extensive volcanic operations, of loose blocks, —and the entire disappearance of streams of lava, would alone imply the great interval of time that had elapsed since their occurrence; but another proof of the same was afforded in the existence of a deep and wide valley, which we crossed on our way, extending from the bottom of the mountain nearly up to the crater, completely covered by vegetation, and with its superficial strata reduced altogether to the condition of a rich and slippery loam.

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[k] Such was the account given me on the spot, but I confess I do not find it confirmed by the narrative of the taking of Melfi furnished by Guicciardini, or by Sismondi.

In the few specimens of volcanic rock met with on our road, either in detached blocks, or composing beds of loose materials, which the soil had not completely concealed from our view, there was a general uniformity of character.

They consisted of a dark, and generally compact, though sometimes cellular, stone, which was made up of an intimate union of minute crystals of augite imbedded in a felspathic base. There were also dispersed through the matrix crystals of a dark green colour, which, from a comparison with others subsequently found, I conclude to be hauyne.

These masses may either have been detached from a stream of lava which formerly had been thrown out, or may have been ejected separately.

If the former were the case, what an idea of antiquity does the complete concealment by vegetation of these streams convey to the mind, when we recollect the ages required to cover over even with lichen those of Etna or Vesuvius!

Yet the whole of these eruptions were posterior to the formation of the tuff constituting the base of the mountain, which is of various degrees of compactness, but for the most part resembles that of Posilippo, containing comminuted masses of pumice imbedded, alternating with beds of loose pumice, without any cementing material.

The rock on which Melfi stands consists of the same substances, but hauyne is more abundant.

The best specimens, however, of that mineral occur in a bed of loose volcanic stones, which lie underneath the lava above noticed. When the crystals are capable of being detached from the matrix, they are generally found to possess the dodecahedral form, which in other localities is seen so imperfectly, well defined, and they are characterized moreover by gelatinizing with nitric acid, and by fusing into a blue glass with borax.

The blue kind found at Andernach, and in the Campagna di Roma, is less common here than the dark green variety, and owing to some external decomposition, the former is white superficially, and presents its natural colour only when fractured. It is accompanied with leucite, with augite, and with hornblende, and in the compact lava-bed which overlies the congeries of ejected masses described, I perceived concretions very much resembling the pearlstone of Hungary.

I am unable to discover, that mount Vultur has ever been measured barometrically, but that its height is very considerable there can be no doubt. At Frigento, which is nearly equi-distant from it and Vesuvius, the summit of Vultur appeared more conspicuously than that of Somma over the Appennine ridge. Dr. Bouè, in his recent work entitled "Guide du Géologue Voyageur," assigns to it an elevation of only 1400 feet, but this is manifestly much below the truth. Its circumference is stated at about twenty-two miles.

It had been my original intention to return to Naples by another road, by Muro, Laviano, Oliveto, and Campagna, which traverses some of the loftier peaks of the Appennines, and would have brought us into that from Naples to Pæstum at Eboli.

But we were dissuaded from this undertaking, by the state of the weather, and the probable depth of the snow on the heights which we should have been compelled to pass.

On the other hand, the river Ofanto was so swelled by the rain which had fallen, as to justify the epithet of *violens Aufidus*, which Horace had applied to it:

> Dicar, qua violens obstrepit Aufidus,
> Qui regna Dauni præfluit Appuli,
> Cum sævit, horrendamque cultis
> Diluviem meditatur agris.

By waiting at Melfi, however, till the flood had in some measure subsided, we were enabled to ford the stream in safety; and our kind friend, the Cavaliere Aranco of Melfi, whose family had been unremitting in their attentions to me during my stay, enabled my Vetturino to drag the carriage through the mud intervening between the town and the high road, a distance of more than thirty miles, by lending me two good mules, in addition to the three horses which we had brought with us. We therefore returned by exactly the same road which had brought us out, and arrived the fourth day at

Naples, where we found a more genial sky, and a climate in every way contrasted with that we had experienced, either upon the Appennines, or round the skirts of that mountain chain.

I have now only to offer some general remarks on volcanos, as suggested by the phenomena which have just been described.

We may, in the first place, point out the existence of a line of country extending from the parallel of Naples to that of mount Vultur, along which volcanic action is developed under most of its different phases, according to certain variations in the external circumstances under which it occurs.

In Ischia, where the line commences, hot springs are abundant, and at distant intervals of time true volcanic operations have been observed.

At Vesuvius, close to the sea, ejections of stones and eruptions of lava are taking place continually; but when we follow the same line across the Appennines, we look in vain for any distinct indication of volcanic action, except at the Lago d' Ansanto, where the same gases are evolved, where a certain degree of heat seems to be generated, but where neither stones nor ashes have ever been thrown out.

Having traversed the Appennines, and descended its eastern slope, we perceive, standing in the same relation to that chain on the one side, which Vesuvius does on the other, a mountain similar in form and structure, but evincing no analogous phenomena at present, except the emission of carbonic acid and sulphuretted hydrogen, and those occasional rumblings of the earth beneath, which have been compared to subterranean thunder, and which, if the natives are to be believed, take place most commonly when Vesuvius itself is in a state of eruption.

With such evidences of an internal communication, or at least of a sympathy between these two distant spots, what circumstances can be pointed out, to account for the almost entire quiescence of the one, and the determination of the volcanic energy to the other? One remarkable difference existing between them is, that, whilst Vesuvius is close to the sea, Vultur is thirty miles apart from it; yet, as we have seen, this difference may not have existed formerly, since the waters of the Adriatic, if they did not actually wash the sides of mount Vultur, once came within a short distance at least of its base.

And, when we examine the present condition of this volcano, we are led to carry back the date of its activity to a period at least as remote as that alluded to; for, so far from imagining it to have been burning in the times of the Romans, its condition seems to have been at that time but little different from that which at present belongs to it.

Horace would scarcely have selected it as the spot, where the doves are represented as covering him over in the days of his childhood with fresh leaves[1], had not the forests existed then in their present luxuriance; and, if the mountain had appeared to the poet under that arid and forbidding aspect which belongs to all volcanos that have been but a short time extinguished, he would have introduced some mention of such a circumstance, when he alludes to the peculiar features of the neighbouring places: the "excelsæ nidum Acherontiæ;" the "saltus Bantinos;" the "arvum pingue humilis Ferenti."

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[1] Me fabulosæ Vulture in Appulo,
Altricis extra limen Apuliæ,
  Ludo fatigatumque somno,
    Fronde nova puerum palumbes
Texere: Hor. Carm. iii. 4.

Lucan too speaks of the "arva Vulturis," without any allusion to the harsh and sterile appearance which belongs to a mass of recently ejected lava.

These indications of an extreme antiquity, compared to that of our earliest historical records, will not surprise those who are at all conversant with volcanic phenomena; and perhaps the extinct volcano of Rocca Monfina near Sessa, to the east of the road from Rome to Naples, furnishes an instance at least as remarkable. In the early history of Rome we read, that a war having broken out between the Sidicini and the Aurunci, the latter, through fear, deserted their city, and fortified Suessa, now called Aurunca, the present Sessa, whilst the walls of their former abode, and all that it contained, were destroyed by the Sidicini.

Some remnants, however, of these walls, I am assured, still exist—they are composed of lava, probably derived from one of the streams described by Breislac as having flowed down the mountain, a proof of their priority to the foundation of a city destroyed 334 years before Christ.

But a more decided proof of the antiquity of this volcano may be collected from the circumstance, that the ancient town stood on the brow of a hill, which constituted part of the margin of this very crater, and consequently that the crater which lies beneath it was then, probably in the same condition as at present, namely, covered with vegetation, so as to furnish the inhabitants with a pasturage for their cattle, and certainly not the scene of volcanic phenomena, which would have precluded them from fixing upon such a site for their abode. Thus the period, at which this Volcano was in a state of activity, must be as remote as that, which we are disposed to assign to mount Vultur[m].

Such then are the volcanic phenomena met with in a direct line, but if we take in a somewhat greater breadth of surface, we shall include, on the side of the Mediterranean, the Islands of Ponza, and on that of the Adriatic, those of Tremiti, off the coast of Manfredonia, whilst the earthquakes under which Foggia and other cities have suffered, shew, that the same operations are carried on elsewhere in the neighbourhood.

In the annexed map I have stated the volcanic districts that occur nearly in a line either to the eastward or the westward of the country described, but I do not wish to lay much stress upon this coincidence of position as a proof of connexion between the more distant points, feeling, that there are perhaps few parallels of latitude, which do not present volcanic phenomena in some part of the globe or another.

I am more disposed to view the Italian peninsula, as part of a long strip of country stretching from north-west to south-east, which is everywhere more or less subject to volcanic operations, but has here and there particular bands of greater intensity running at right angles to the former direction, or nearly from north-east to south-west. The first might be represented by a longitudinal band, the second by transverse lines intersecting it. Thus let a a a a represent the longitudinal band running from north to south of Italy, and thence extending to Sicily, and the transverse line B B will denote the line of greatest intensity stretching across Tuscany; C C that stretching across the Romagna; D D that from Naples to mount Vultur; E E that from the Lipari islands to the theatre of the Calabrian earthquakes; F F that from mount Etna to the new island of Sciacca.

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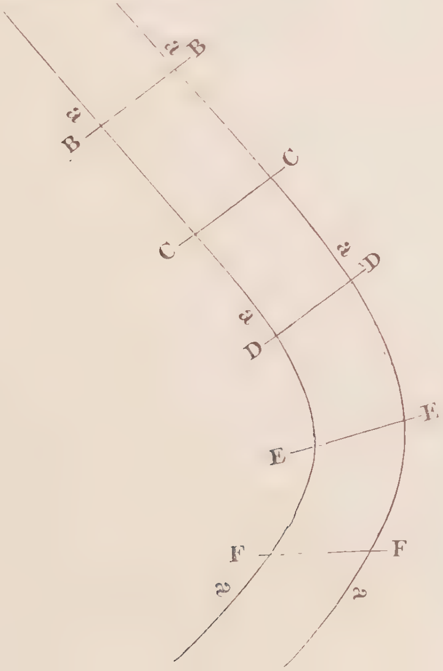
[m] From a statement of Breislac's which I have quoted in my "Description of Volcanos," p. 137, it would follow that Rocca Monfina had been in action during the historical period, for it is there mentioned that the tuff on which the town of Sessa is built covers the remains of an ancient city, and amongst the rest, the vestiges of an amphitheatre. In my last visit to Italy I examined the spot, and found the buildings alluded to, but the amphitheatre, though considerably below the present town, seems to have been built in a natural hollow in the tufaceous rock, and never to have been covered over by that deposit; neither do the remains of another ancient building that has been detected, bear out Breislac's conclusion, which is moreover encumbered with so many difficulties, as to require the most overpowering evidence to establish its credibility.



APPENDIX.

Note at page 10.

I CANNOT, however, omit this opportunity of noticing two varieties of volcanic tuff which appear not to have been sufficiently studied, and which shew, that since the original ejection of the materials, their deposition at the bottom of water, and their consolidation into a mass of uniform consistency, changes have taken place in them, which the long continued operation of a moderate heat is perhaps most likely to have produced.

The first of these varieties is seen in the valley of St. Rocca, near the Capo di Monte, above Naples, where the tuff has been extensively quarried, and where large caverns are left in the rock, which, by exposing a considerable extent of surface, allow of our examining the structure on the great scale.

The most remarkable circumstance relating to it, is the existence of veins of a harder variety of the same rock traversing the bed. I call the matter of the veins a variety of tuff, because in some cases there seemed to be a passage from the ordinary kind into it, though in others the line of demarcation is very distinct.

In its hardest form it approaches nearly to porcelain jasper, is splintery, and has a conchoidal fracture. In these cases its surface is smooth, and its texture uniform, but when it is traced on, we discover in it a fragmentary structure, bits of obsidian, porphyry, pumice, and lapilli of various sizes and kinds being imbedded in the same paste. In other instances little veins of the same fragmentary variety of rock pass through the substance of the compact and uniform kind. The harder varieties of tuff, here described as veins, contain specks of mica disseminated through them.

Its surface is sometimes coated with a white incrustation, which seems to be siliceous.

It is seen to rest on a bed of the same volcanic materials, possessing however a very loose consistence, and composed principally of a congeries of grains of sand, and of lapilli.

Of this modification of tuff I know of no notice having been taken, excepting by professor Tenore of Naples, in his "Essai sur la Geographie Physique de Naples;" but the following is alluded to by Breislac and others under the name of Piperno, the name applied to it from one of the places where it has been found.

The spot in which I principally examined this latter variety, was the north side of the crater of Pianura, five miles from Naples. Like several other bason-shaped cavities that occur in the great tufaceous deposit overspreading that country, to which from the similarity of their form we are disposed to attribute the same origin, the sides of this crater exhibit no alternation of beds of lava and of ejected materials, or other traces of having been at any time the theatre of those eruptions of lava which occur at Vesuvius at present. They are composed indeed of the same volcanic tuff as that of Posilippo, with which this bed seems continuous, but the rock, which bounds the crater towards the south, for a certain distance upwards from the level of the hollow within, presents that character which has caused it to be designated by the popular name of Piperno.

Being extensively quarried for building purposes[a], galleries have been carried for a great distance into the rock, both horizontally and perpendicularly, and by means of the latter it has been ascertained that the same material extends at least forty feet beneath the level of the bottom of the crater. With respect to its external characters, Piperno may be described as a sort of volcanic breccia, bearing the same relation to the ordinary tuff, which Sienna marble does to an ordinary puddingstone, containing the same fragments of volcanic matter as the latter, but with the imbedded fragments so blended with the cementing material, as to appear like a part of the same.

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[a] It is used for the steps of houses, the sills of windows, &c. being more durable than tuff, and more easily cut than lava.

These fragments, which are sometimes without, but more commonly with cells, are often compressed and lengthened out in an horizontal direction, and are imbedded in a whitish paste, similar to ordinary tuff, but more compact. Indeed the beds of tuff that occur above it resemble much, except in hardness, the basis of the Piperno.

It is interesting to observe, that just over, and, as it were, passing into, this rock, is a tuff, with fragments of these several kinds of lava abundantly scattered through it, but existing in a state of very loose aggregation, excepting in the vicinity of the Piperno, where it becomes more and more compact, gradually passing into a perfect breccia. It is said moreover, that the best and hardest Piperno is taken from the bottom of the bed.

The impression which the appearances of Piperno created in my mind, was that of a bed of ordinary tuff, which after it had been originally deposited, was subjected to the action of heat under pressure, and in this manner underwent a kind of partial fusion. Under this condition of things, crystals of felspar, which are now abundant in it, as likewise the other minerals we occasionally meet with, although they did not previously exist, might be formed, and by the same agency the fragments disseminated through it might become blended with the surrounding matrix in the manner we find them to be.

There is yet another variety of volcanic tuff known in the Campagna di Roma and elsewhere, under the name of Peperino, which must not be confounded with the Piperno already described.

There is, however, thus far a correspondence between them, that Peperino is also a material which appears to have undergone certain intestine changes since it was first deposited, the effects of which have been to produce in the midst of the amorphous mass certain crystalline minerals, such as mica, augite, felspar, &c., and to impart a more uniform degree of compactness to the whole.

Whether these changes resulted from heat subsequently applied, may admit of dispute, but that here also some chemical operation or other has produced a fresh arrangement of the particles since the mass was originally formed, will scarcely be doubted by those, who admit that Peperino is a modification of the volcanic tuff found in its vicinity.

Yet even the latter, as seen at Posilippo, is more compact than the material known under the same name which has covered over Herculaneum, or which has resulted in other instances from the action of rains and torrents upon showers of ashes proceeding from modern eruptions.

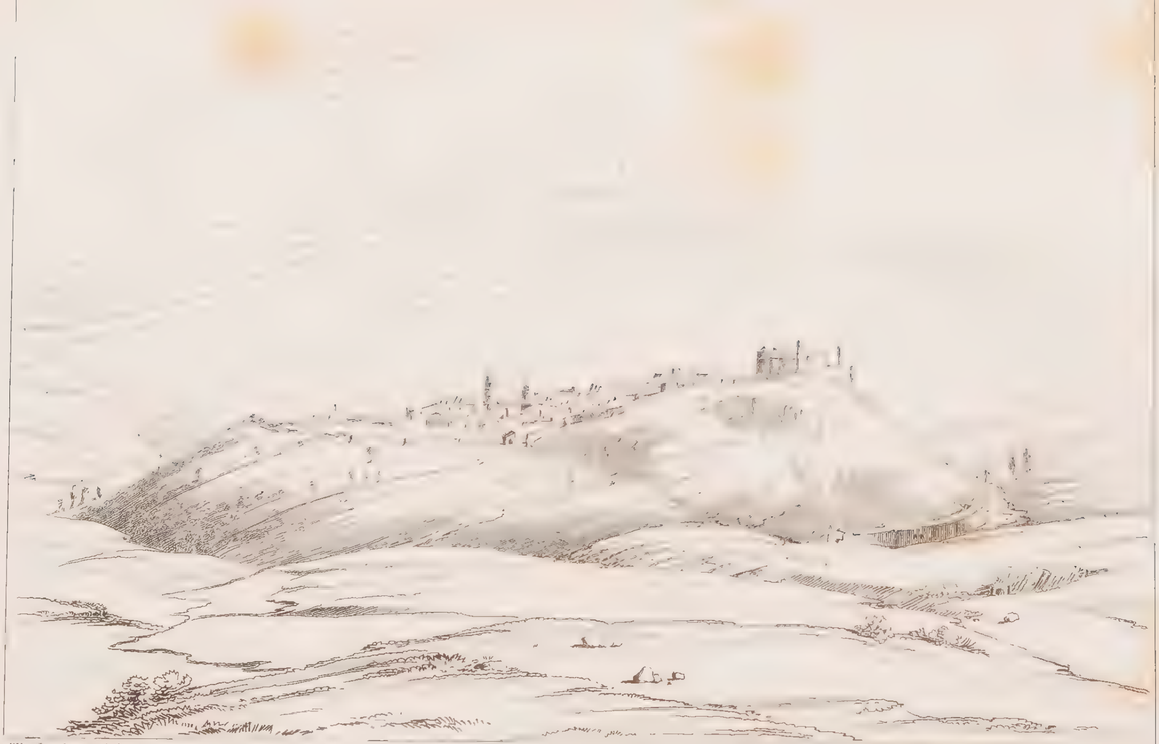
Not that there is any original difference between the materials ejected, but that in the former instance the pressure of so much larger a bulk of matter, and probably also of the body of water, which for a time covered it, has caused a more intimate union between the several parts, and given it more of the character of an homogeneous rock.

Post Road to Apulia.

	Post.*
Naples to Marigliano	1½
(Thence to Nola, ½ post.)	
Marigliano to Cardinale	1½
Cardinale to Avellino	1½
Avellino to Dentecane	1½
Dentecane to Grotta Minarda	1½
Grotta Minarda to Ariano	1
Ariano to Savignano	1
Savignano to Ponte di Bovino	1½
(Hence by the cross-road, Melfi is about 5 posts distant.)	
Ponte di Bovino to Pozzo d' Albero	1
Pozzo d' Albero to Foggia †	1
Foggia to Passo d' Orta	1½
Passo d' Orta to Cirignola	1
(A bridge over the Ofanto, and a bridle road to Melfi.)	
Cirignola to S. Cassano	1
S. Cassano to Barletta	1
(Beyond Cirignola lies the field of Cannæ.)	
Barletta to Bisceglie	1
Bisceglie to Giovinazzo	1
Giovinazzo to Bari	1½
Bari to Mola	1½
Mola to Monopoli	1½
Monopoli to Fasano	1
Fasano to Ostuni	1½
Ostuni to S. Vito	1½
(Hence to Brindisi 1 post.)	
S. Vito to Mesagne	1½
Mesagne to Cellino	1½
Cellino to Lecce	1½
(Hence to Gallipoli 3 posts.)	
Lecce to Martano	1½
Martano to Otranto	1½

* N. B. A post is between 6 or 7 miles, but the Neapolitan mile is about 1¼ English.

† A Diligence twice a week from Naples to Foggia.

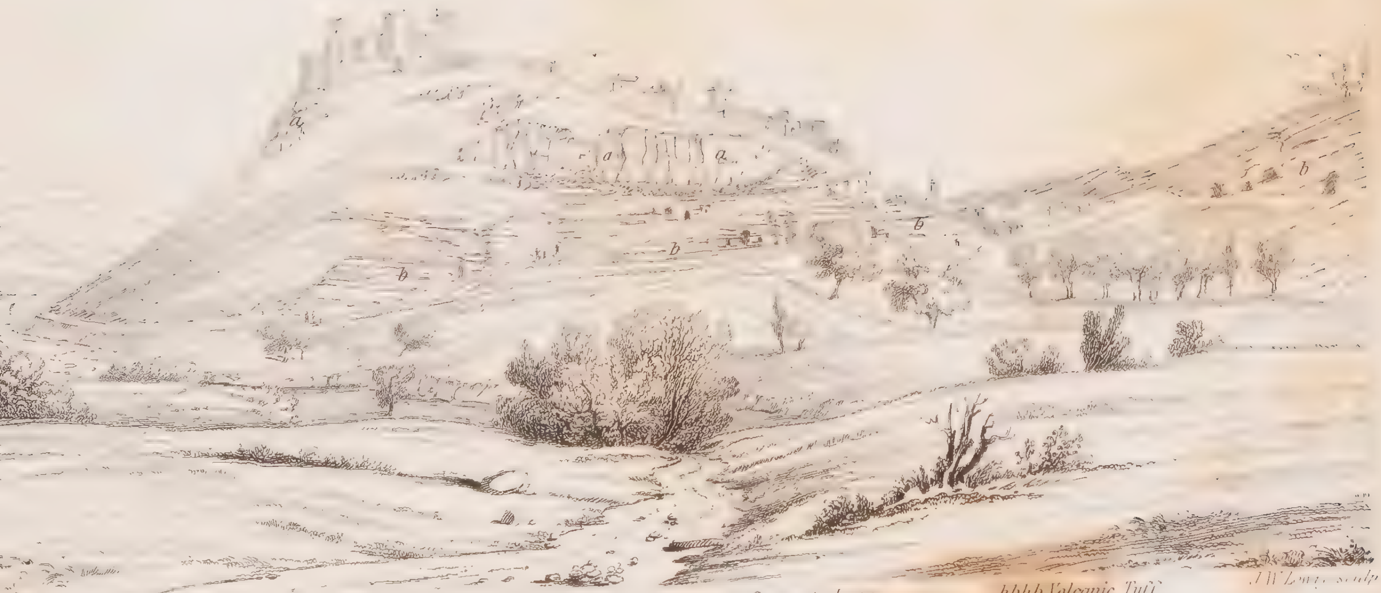


Th. Jueler. delin

J.W. Lowry sculp.

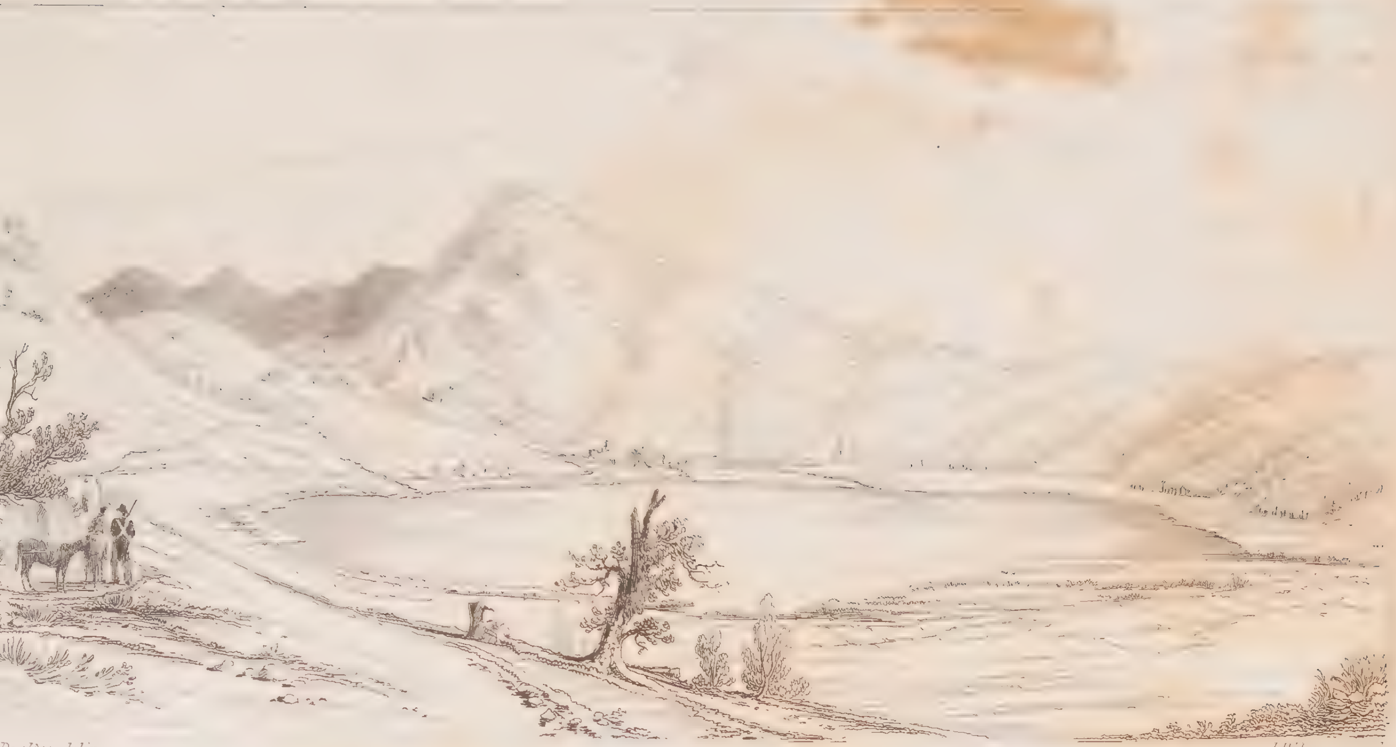
TOWN & CASTLE OF MELFI WITH M^T. VULTUR BEYOND,

SEEN FROM THE N.E.



Duclère, delin. Rock of Melfi. Mount Thabor. a.a.a. Compact Lava. b.b.b.b. Volcanic Tuff. J.W. Lowry, sculp.

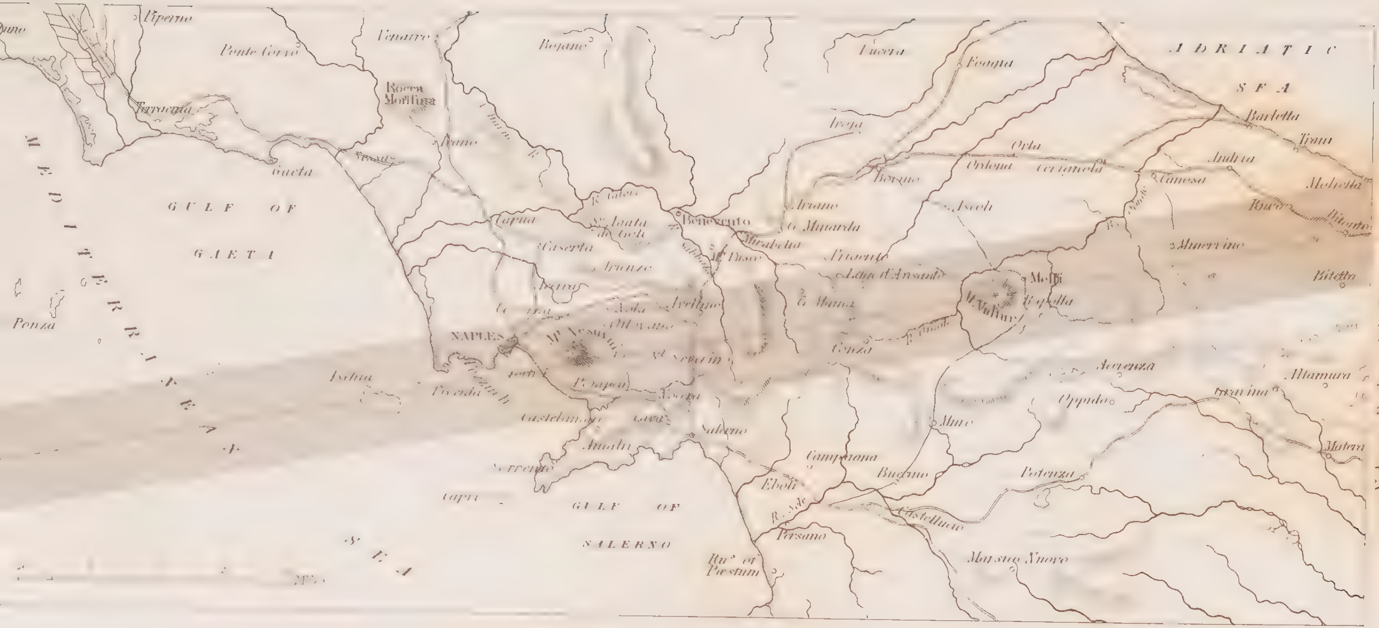
TOWN & CASTLE OF MELFI.



Duclère, delin

J. W. Lowry sculp

CRATER OF MOUNT VULTUR FROM THE S. W.



J W Lowry sculp

MAP OF A PORTION OF THE KINGDOM OF NAPLES.

Volcanic Rocks of the Tremiti Isles 40°
the Bosphorus 10° to 11°
Lemnos & the Troad 40°

ON THE

THEORY

OF

RATIO AND PROPORTION,

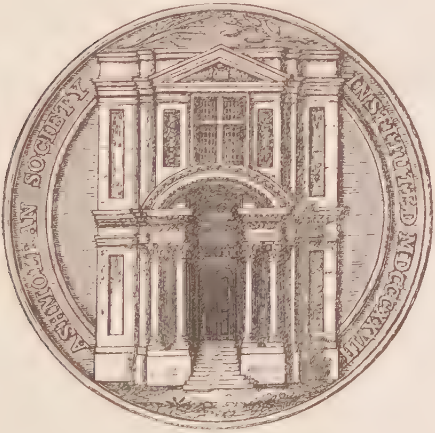
AS TREATED BY EUCLID,

INCLUDING AN INQUIRY INTO THE NATURE OF

QUANTITY.

BY

THE REV. BADEN POWELL, M.A. F.R.S.
OF ORIEL COLLEGE,
AND SAVILIAN PROFESSOR OF GEOMETRY.



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MDCCCXXXVI.

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MATHEMATICA MULTA SCIUNT; MATHESIN PAUCI.

BARROW.

“The perusal of the opinions of an individual, offered simply “as such, may excite many to become inquirers who would “otherwise have been workers of rules and followers of dogmas: “they may not ultimately coincide in the views promulgated by “the work which first drew their attention, but the benefit “which they will derive from it, is not the less on that account.” (Study of Mathematics; Preface; Library of Useful Knowledge.)

ON THE

THEORY OF RATIO AND PROPORTION,

AS TREATED BY EUCLID,

INCLUDING AN INQUIRY INTO THE NATURE OF QUANTITY.

READ TO THE ASHMOLEAN SOCIETY, FEB. 5, 1836.

Introductory Remarks.

QUESTIONS relating to those first principles in which all mathematical truth originates are at once of considerable importance, difficulty, and philosophic interest. Yet in each respect they are commonly underrated, and inquiries which really involve the stability of the very foundations of the science are too often either wholly neglected, or receive so slight, or so confused, a discussion, as leads to the most vague, inconsistent, or fallacious views of them.

Some mathematical writers, professing to lay down the elements of the science in a strictly logical method, yet disregard such considerations altogether: and others even of the highest celebrity evince a singular infelicity in the way in which they reason on them. Once, indeed, fairly embarked in the actual processes of mathematical investigations, their ability is soon conspicuously displayed: but in the more humble, though not less important department of fixing the first conceptions, and scrutinizing the evidence of first principles, they have in many instances left the subject not only without adequate explanation, but even placed in a very exceptionable point of view. Perhaps indeed, the peculiar genius for pursuing a train of mathematical deduction may be of a distinct character from that which delights in the analysis of first principles: and the two may be seldom found combined. Certainly the two species of research, require, and promote, habits of investigation of very distinct kinds. Instances are not wanting, of those who have displayed the highest powers in perceiving, as it were by intuition, the most complex relations of magnitudes, and in comprehending at a glance, the remote consequences of all the varied combinations of quantity, and yet the same minds have been the least disposed or fitted, for estimating those precise, yet simple distinctions, by which the soundness of the very foundations of the science is to be tried. These discussions, to the success of which, no skill in the combinations of theorems or in the management of symbols, can contribute, are often avowedly passed over as not properly belonging to the province of the geometer or of the analyst. And such questions, thus too often rejected by the mathematician as metaphysical, are on the other hand discarded by the metaphysician as mathematical: so that, disowned by both, they fall to the ground without any satisfactory discussion at all.

And even if such an investigation be prosecuted, it is likely to receive but little attention. To the advanced mathematician, the discussion of such purely elementary topics is almost invariably uninteresting, or even repulsive. The mental satisfaction arising from the beautiful succession of results which the higher branches present, displaying themselves in a new light at every turn, and the charm arising from the surprising coincidences between different trains of deduction, are intellectual gratifications of so high an order, that those who have experienced them to any great extent, can seldom recall their thoughts without much reluctance to the minute analysis of first principles. While, on the other hand, the more elementary student is by no means anxious to add to the difficulties which already beset his path, by entering upon new fields which not only appear to lie out of his way, but present a dull and uninviting aspect.

Perhaps, however, in a society like ours, and among the members of an university where logical studies occupy so large a share of attention, while the elements of mathematics are avowedly pursued, more with reference to the principles of reasoning, than to the prosecution of higher results, speculations on the points alluded to may hope for a more favourable reception.

General Statement of the Question.

On an attentive examination of the methods adopted by modern elementary writers, in laying down the first principles of ratios and proportion, and especially in commenting upon Euclid, I long since experienced a conviction of the extremely unsatisfactory nature of most of their views; and this chiefly, as appearing to me to involve inadequate ideas of Euclid's real principle in treating of proportionals in his 5th book, and of the nature of the quantities which form the subject of investigation.

In the endeavour to supply (at least in some degree) an elucidation of the subject, I now submit my speculations to the consideration of the Society. Upon a brief review of the leading opinions which have been advanced, I propose to deduce a view of the nature and primary relations of quantity, which, though I believe it will be found to accord with what can be collected of the ideas of some of the most profound geometers, is yet not distinctly indicated by them: while opinions of a very opposite and (as appears to me) erroneous nature have been held by others not less eminent.

The doctrine of ratios and proportion is introduced by Euclid as a part of his system of geometry; and the student seldom fails to remark, that in the treatises on algebra, the same subject is presented under a considerably different form; though he is usually quite unable to determine wherein the essential difference consists; and would probably find but few teachers who could precisely point out the distinction to him. Some of the best modern writers on geometry (as Legendre) omit it altogether in their elementary systems: and most teachers in this country, though in general they follow Euclid, yet pass over the 5th book, and adopting the doctrine of proportionals from algebra, proceed to apply it to the theorems of the 6th book. Euclid however, for some reason, thought it necessary to proceed otherwise; he establishes by a totally different method some of the same properties of proportionals as the algebraic writers do: but where he stops short, (not even proceeding to certain of the simplest and most universally important properties,) they on the other hand continue the subject, or make those properties fundamental. Of Euclid's design and principles of investigation, various opinions have been held among the moderns; and some of those who most profess to be his admirers and followers have made attempts, as they conceive, to improve upon his method; and have devised various plans for treating this portion of the subject, in order to avoid what they consider the unnecessary abstruseness and prolixity of the 5th book.

It might perhaps in the first instance be imagined, that the method adopted by Euclid in his 5th book was the first and imperfect attempt of science, as yet in its infancy, to give a general investigation of the doctrine of proportion; which, however prolix and cumbrous, ought yet on that ground to claim our respect and admiration; but that the advance of modern science, having furnished us with the more comprehensive method of algebra, we may properly discard the older and more difficult process, as doubtless Euclid would have done, had the better method been known to him.

But what is the fact? We have only to look into his 7th book, and we there find the whole doctrine as applied to the case of *numbers* demonstrated quite independently of any thing in the 5th book, on principles, though presented in a different form, yet in substance almost identically the same as those used in modern algebraical treatises. And it would have required no more than a very obvious extension of the method, so far to generalize it as to make it applicable to the geometrical figures considered in the 6th book, in the same way as the modern writers have done.

Euclid's method. Commensurables and Incommensurables.

To follow up the inquiry we must examine a little more closely into Euclid's system.

If we look back to his 2nd book, we find there a subject, in reference to which, the methods of geometry and algebra are brought into comparison and connexion, by the investigations commonly given, either as a comment upon, or substitute for, the demonstrations of that book, dependent on the analogy subsisting between the *areas* of rectangles and the *products* of the algebraical quantities by which their sides may be represented. And if a comparison be instituted between the geometrical and algebraical modes of proving these theorems, it requires little discernment for the student to perceive, that this application of algebra turns essentially upon two assumptions: first, that the sides may be conceived divided into a number of equal parts, one of which is considered the linear unit: second, that the square described on one of these linear units is to be regarded as the superficial unit. If then the two sides each contain an exact number of such units, upon these principles, it follows that the area of the rectangle is represented by the product of the numbers expressing the sides; and thence all the propositions of the 2nd book (with one exception) receive an immediate proof by the mere process of multiplication. But a very scrupulous and reflecting student will feel some hesitation at this view of the subject: for how, he will ask, can we assume that *each of* the two sides will always contain an *exact* number of such units? they *may* do so; but the theorems of the 2nd book suppose lines divided into *any* two parts whatever; and if we assume any given line as the unit, it is obvious that innumerable cases may be supposed where this unit, though contained exactly a certain number of times in one of the lines, will not be so in the other. The obvious resource is to take a smaller unit; or to suppose the first unit subdivided, suppose decimally: then its tenth perhaps may measure them both exactly: or if not the tenth, the hundredth may; thus the process may be continued till a common measure is found; but to make it absolutely general, we are necessitated to suppose this process carried on *ad infinitum*. All this, if we are to stop to demonstrate it, will involve us in very abstruse considerations. Yet it generally happens that the ordinary learner finds no difficulty in admitting such ideas: and this arises from the familiar use of arithmetic, when we are early habituated to the notion of a result, *with a small remainder*. By the decimal system we so readily express our result to any degree of accuracy requisite, that for all practical purposes we adopt approximate numbers as sufficiently exact.

The transference of this idea to the investigations of mathematics therefore creates no embarrassment in the mind of the learner; he is satisfied with the algebraical proofs of the propositions in the 2nd book, even without taking the trouble to enter into the above considerations, by the aid of the mere illustration of whole and finite numbers. In the 11th prop. indeed, where the solution of a quadratic is necessary, and the result appears involving a numerical root, it is rendered palpable that the quantities concerned are essentially such as cannot be both expressed in finite numbers, or, are incommensurable. Of all this however nothing appears in Euclid's mode of treating the subject, and it arises wholly out of the adoption of the algebraical processes.

Thus, various relations being established in geometry between lines constituted under given conditions, as parts of geometrical figures, *if* we choose to adopt the idea of expressing these lines by *numerical measures*, we are then brought to the distinction of such lines being in some cases commensurable in their numerical values, in others not so. Their *geometrical relations* however are absolutely general, and do not refer to any such distinction.

Euclid, in his earlier books, when treating of such properties as those alluded to, does not introduce either explicitly or by implication the term or the idea of incommensurability: neither is it introduced in the 5th or 6th books. After having in the 7th treated of the properties of arithmetical proportionals where the terms are all supposed expressed in exact finite numbers, and having in the 8th and 9th books continued this subject to various properties of numbers, he comes in the 10th book, for the first time, to introduce the notion of a distinction between commensurables and incommensurables, in geometrical magnitudes expressed by numerical measures. In the 11th and 12th books again, he drops all reference to this distinction, recurring to the principles of the 5th book: but resumes the consideration of it in the 13th, and applies it to various properties.

Much of the confusion of ideas which has arisen on these subjects has been occasioned by not observing that when we say "two lines are incommen- "surable," the phrase is in fact elliptical, and we ought always to consider as understood, if not expressed, that "two lines, *if referred to numbers*, "are incommensurable." The deficiency of exact comparison in such cases is not in the *geometrical* relation of the quantities, but in the powers and capacities of our *numerical* system to express them.

These observations may be necessary to enable us to appreciate better the opinions which have since been prevalent among mathematicians; more especially as regards the doctrine of proportionals.

Views of the earlier Geometers.

From the time of Theon downwards there appears to have been a prevailing disposition among the commentators of Euclid, not only to put their own expositions on his methods in such a way as to involve them in no small perplexity and obscurity, but absolutely to discard his definitions, and substitute other views of the subject which they imagined more luminous. Among the more modern writers we find a variety of objections started against Euclid's method, and a perpetual desire to introduce what they considered preferable modes of investigation, chiefly, as appears to me, from not well understanding those which they criticised.

Tacquet censured Euclid's definition of proportionals as turning not on an essential but on an accidental property. He accordingly in his own edition accompanies it with a comment, which puts the subject on the basis of numerical multiples.

Gregory Vincent adopted an explanation tantamount to the numerical principle; considering ratios as themselves quantities.

Hobbes objected to the 5th definition as involving the idea of taking *any multiples whatever*, which he contended is an inconceivable supposition.

Borelli found fault with it as obscure, and as it is not evident that there are any such quantities as would fulfil the conditions. He held (as others have done since) that Euclid himself thought his own view of the matter insufficient, because he gives another in the 7th book. He also blames Euclid for not in this place explaining incommensurables.

Dr. Barrow[a] in his celebrated Mathematical Lectures for 1666 has treated the whole subject of ratios and proportion in the most copious and elaborate manner, but, as appears to me, with more learning than perspicuity: he is extensively occupied in examining and refuting such objections as those just adverted to; and in doing so seems more explicit and satisfactory, than in any attempts to elucidate directly the doctrine itself on real philosophical principles. In point of fact, in the midst of his very extensive dissertations, it is far from an easy matter to discover precisely what is his own idea of the nature of ratios: and when it is developed, it is by no means clear wherein it substantially differs from the views of some of his opponents.

From those passages which may be regarded as most explicit, it appears that he considered the notion of commensurability and incommensurability as *necessarily* entering into the doctrine of proportion: and that his idea of proportion is founded upon the consideration of the antecedents being the same multiples or parts of their consequents: this applies exactly in the case of commensurables; and by approximation to any required degree of accuracy incommensurables may be in like manner subjected to the same mode of comparison: thus our view of proportion must be conveyed in such general terms as to include both cases.

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[a] See note A.

This is in effect to estimate all ratios by numbers; a view of the subject which is certainly in itself sufficiently definite and perspicuous, but which if it had but been explicitly stated at the outset would have removed greatly the obscurity in which the discussion appears involved. It would almost seem as if there were some reluctance on the part of the author to avow it: and it must on all hands be confessed that there is a considerable difficulty remaining, viz. to shew whether this was Euclid's view of the subject; if so, why he adopted the method pursued in the fifth book? if not, what was his view of the matter?

Dr. Barrow discusses at considerable length the definition of "ratio," given in the fifth book, as well as that subsequently occurring of "analogy;" and concludes by designating these definitions as metaphysical, since nothing, he conceives, in mathematics depends upon them. He thinks they were introduced with the view of giving learners some general though vague notions on the subject before proceeding to the more accurate investigation of it which follows.

Opinions of the later Mathematicians.

Among writers of more recent date, though we find continual eulogy and commendation bestowed upon Euclid's method, yet the praise thus awarded in words is not unfrequently nullified in fact, by their substitution of other methods, which, under the name of improvements, are in truth entire subversions of his system.

The arrangement of Euclid is censured, because, it is alleged, the learner can neither understand the full force of the 5th def., nor comprehend what could induce the author to adopt so artificial a criterion of proportion without a knowledge of incommensurables: which subject is nevertheless not touched upon till the 10th book.

Such is the explicit representation of one eminent writer, and many others have joined in the same views[b]. I fail, however, in perceiving the force of the reason assigned for considering Euclid's arrangement defective. I neither understand why it should be necessary that a learner should apprehend "the full force" of a definition, nor indeed what that expression means, unless that he ought to be able to enter into all the author's reasons for adopting one characteristic on which to frame his definition in preference to another; a sort of knowledge which it is hardly necessary to say is neither supposed in reference to any definition in other parts of mathematics, nor, from the nature of the case, could it in general be attained, or intelligibly communicated; nor, it may be added, would it be of much use if it could. All definitions are essentially arbitrary; and all that the student has to do is to confine his attention strictly to the sense which his author imposes upon a particular term, and to be careful to infer nothing but what is implied in the meaning so affixed to it. And in relation to this particular definition, the difficulty lies obviously and solely in the somewhat complex notion which it presents as the test of proportionality, and which the student has to bear in mind, and apply in the subsequent propositions. But this difficulty would in no degree be diminished by further entering upon

[b] See note B.

the not less complex considerations of commensurables and incommensurables, and the whole system of numerical measures, which form the reason alleged for adopting the definition; and which if here introduced, could hardly produce any other effect than to increase the perplexity of the learner; and all this supposing the reason alleged to be the true one, which we shall examine presently.

Conceiving, however, that such a course would be necessary for giving the student a complete notion of the subject, and also, I suppose, admitting that it would be hardly practicable to attempt to adopt it, these writers have preferred the entire rejection of Euclid's method, and the substitution of one founded on the principle of numerical comparison.

They accordingly commence with a few theorems, in which they treat of the simpler geometrical cases where incommensurables occur; and after defining ratio as the relation of quantities, one of which is considered as composed of parts of the other, they explicitly maintain that number enters *essentially* into our conception of ratio, which is in fact nothing more than the expression in numbers of the relative quantity of two magnitudes of the same kind. They then distinguish the cases of quantities which are incommensurable, and demonstrate the principle of approximation, by which we obtain the means of subjecting them with sufficient exactness to the same mode of numerical comparison. Thus they establish the properties of proportionals in a way which is in fact the same as that adopted in the common treatises on algebra. The circumstance of their system being formally enunciated in distinct theorems, and composed in the style of the ancient geometers, constitutes of côurse no essential distinction.

Sir J. Leslie's view of the subject.

Almost exactly coinciding with these views, (as well as with those of several later writers,) are those of sir John Leslie[c]. His "Elements of Geometry" is a work professedly formed upon the most exact model of the ancient methods: yet here the idea of *number* is explicitly maintained as entering *essentially* into our idea of ratio; and a distinction is drawn between the notions of quantity as contemplated in the earlier parts of geometry, where it is said to be "concrete," and in the later, (the 5th and following books,) where it becomes "discrete;" and "geometry is brought under the dominion of arithmetic." And upon these principles the author founds an investigation of proportionals almost identical with that of the last mentioned, and many subsequent, writers. Now certainly if this primary idea of quantity and of ratio be adopted, the method of these authors is perfectly consistent throughout; and if Euclid had entertained the same view of the subject, he had all the materials at his disposal, and would only have had to arrange them in a different order, following nearly the same plan as that adopted by these writers. But, as was before observed, the recognition of any distinction between commensurables and incommensurables arises solely out of the adoption of those numerical measures of quantity which these mathematicians have not only made the basis of their definitions, but have regarded as essentially involved in all our ideas of ratio. It is this fundamental point then which requires further examination.

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[c] See note C.

These authors have undoubtedly a right to adopt this view of the nature and quantity of ratio: but in asserting that it is *essential* to our very idea and conception of the subject, they are asserting what they have certainly not proved, nor attempted to prove, and what appears to me absolutely untrue.

But further;—if this principle of a numerical estimate of magnitude be assumed at one point in the course, why is it not employed from the first? This would not only render the whole uniform, but would immensely abridge the whole investigation of rectangles. Why, it may be asked, is "geometry "brought under the dominion of arithmetic" only at this particular stage? why are lines and surfaces to be considered as concrete before and discrete only now? and if the investigation is so largely facilitated by becoming arithmetical here, why is not this facility allowed in the earlier parts, where it would be so much more desirable? or, lastly, if expression by number be essential to our exact ideas of quantity, what ideas are those which have been adopted throughout the preceding investigation? and if to have exact ideas, it is essential to consider quantities as *discrete*, what idea have we at all of a *concrete* quantity? It is maintained by these writers, that Euclid's object was to give such an investigation of proportionals as should include both commensurables and incommensurables. Yet he is accused of "a certain indefinitude of principle," and of "evading rather than fairly meeting the difficulties "of the subject."

Now deferring for the present the question whether Euclid's real, sole, and specific object was as here stated, I would ask what is this "indefinitude of "principle" alluded to? Recondite and abstruse the method followed in the fifth book may possibly be called, but surely not deficient in precision or definiteness. What again is meant by evading the difficulties of the subject? when every student complains that the method pursued is peculiarly beset with difficulties, and when it is implied in the system proposed to be substituted, that Euclid has rather unnecessarily involved himself in them. Whatever difficulties there may be in the subject, I believe the last accusation which can reasonably be brought against Euclid is that of an unwillingness to grapple with them.

If by evading the difficulties of the subject be meant avoiding the introduction of any reference to the distinction between commensurables and incommensurables, the charge is certainly true; but a very little careful examination will shew (as I hope soon to evince) that there were other good reasons for this studious anxiety to avoid such considerations, widely different from any wish to evade difficulties.

But Euclid is further accused of want of due consistency in his plan; and of wavering between one view of his subject and another: as if in adopting the definition of the 7th book he must be understood to retract and abandon that used throughout the 5th and 6th.

But what is the real state of the case? He frames a definition of proportion to apply to those magnitudes which are the subject of consideration throughout the 5th book, and which are applied in geometrical constructions in the 6th. And if he abandons this definition in the 7th and following books, it is only to resume it again when he proceeds to plane and solid geometry in the 11th and 12th books, (as e. g. prop. 25. book 11,) where he recurs directly to the 5th book without any reference to the intervening books.

In the 7th book it is true he adopts another and simpler definition of proportion; but this is as subsisting among quantities of a particular species which he had not before treated of, and which from their nature admit of a more simple view of the ratios between them.

In what sense this can be said to betray inconsistency is not easily apparent. But further: this last is *not a new and distinct* definition, grounded upon an independent property; but on one which is simply a particular case of the former more general property; and is in fact deducible from it, as appears in props. C and D, inserted by Dr. Simson in the 5th book.

We have here supposed admitted for the sake of argument that Euclid's specific object in the 5th book was to include incommensurables. That in point of fact his method is such as does include them is undoubtedly true: but the question is, was his object *limited* to this design, or even at all *directed to it as such?*

His immediate object manifestly was to investigate the doctrine of proportionals, in the 5th book, in such a way as to apply directly to the geometrical properties of rectilinear figures in the sixth, and of surfaces and solids in the 11th and 12th books. Now certainly these theorems, to be given in a sufficiently general manner, must be conceived and demonstrated on such principles as shall comprehend the cases where the lines may be incommensurable: and some among them refer to lines always incommensurable.

But if Euclid had merely as his object to establish these properties in such a way as to include incommensurables, he must assuredly have seen, as well as his modern commentators, that he might have effected this by following just such a course as they have traced: that is, by a mere change in the order of his books, which would have enabled him to omit the 5th book altogether. And it is extraordinary, that if he had such an object he should never have given the smallest hint of it, or have afforded the slightest assistance to the learner in seizing so valuable a clue to the intricacies of the subject.

The real object of Euclid's system was indeed such as to *include* the relations of quantities which, *if a particular standard* of measurement were applied, might be found either commensurable or incommensurable *by that standard*. His method is undoubtedly framed so as to include both: but so to include them as at the same time to avoid naming them; to investigate general theorems in such a way that they should really be found to comprise certain particular cases, whenever such distinct cases should be alleged: but to investigate them in such terms, that there should be no occasion to mention those particular cases, or even to imply their existence as distinct cases. The student in fact might thoroughly study the six books without having the smallest notion of such a distinction subsisting.

The authors referred to, complain of Dr. Simson and other modern editors for retaining the 5th book, and yet omitting all the books relating to the properties of numbers; and not adverting even to the existence of incommensurables, which, according to their view, the 5th book was framed to include, but which are not explained nor even alluded to till in the 10th book.

Now it is quite clear that Dr. Simson omits those books which treat of *numbers* and of all the properties of geometrical figures which involve numbers, simply because they are absolutely unconnected with the geometrical books: and moreover, all the important truths demonstrated in them are attainable by the modern student with greater facility in the study of algebra. The omission of incommensurables would certainly be a fault had any discussion of numbers been admitted: but without this, so far from being essential, it could not properly have found any place in a purely geometrical system.

Some argument has been raised upon the definition of "ratio" which had held a place in all the older versions of Euclid; and though not omitted by Dr. Simson, is yet considered by him to be spurious, and probably inserted by Theon or some other unskilful editor, who did not understand Euclid's design.

Sir J. Leslie, however, anxiously retains it; and from a critical interpretation of the original words, viz. that it is a relation *κατὰ πηλίκοτητα*, maintains (as others had done before him) that it involves essentially the idea of *number;* [p. 335.] and thus finds a sanction for his own view of the subject [d].

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[d] See note D.

The definition thus understood, (to say nothing of the philological merits of the interpretation,) is absolutely at variance with the whole tenor and character of Euclid's investigations throughout the 5th book: in which it is notorious he studiously avoids any introduction direct or indirect of the idea of number as connected with the relations of those quantities whose ratios are compared.

More particularly, if he had adopted such a definition of ratio, how can it be explained why he should not have framed the definition of proportion upon the same principle, as he might easily have done, and as the modern writers have done? He might have "brought geometry under the dominion "of arithmetic;" but he has obviously not thought fit to do so. He might have gone upon the principle of this definition, which in fact if only consistently followed up would have rendered utterly superfluous the definition of proportionals which he has given, and the whole of that refined and artificial mode of establishing their properties, which constitutes at once the characteristic excellence and difficulty of the 5th book.

Other Objections to Euclid's Method considered.

This may also be the proper place to call the attention of the reader to one circumstance especially observable in Euclid's definition of proportion; which, though obvious as to the fact, requires to be carefully borne in mind with reference to certain objections, viz. that the characteristic fixed upon, on which to frame that definition, is *nothing intrinsic to the nature of the quantities themselves*, but the result of a certain operation supposed to be performed upon them. He does not make the nature of proportion depend upon any relation existing between the *quantities themselves* considered in itself, but only upon a relation subsisting between certain other quantities, which *result* from a particular process applied to the original quantities. Thus the notion of *multiples* and *any other notions* implied in the process of taking those multiples, are notions *not involved essentially in our idea of the nature of proportionals, but* EXTRINSIC *to that idea,* and introduced as the *SUBSIDIARY* means by which we are enabled to give such a definition of proportion as shall serve for the basis of our future reasonings. In a word, the notion of *multiples* is involved in the *process* by which Euclid frames and applies his test of proportionality, *but not at all in the idea of the proportionals themselves.*

Perhaps it may be thought that this is dwelling too long upon what is self evident: but the fact is, we shall find it necessary to recur to this distinction; as it is sometimes lost sight of, and considerable difficulty and obscurity arise from the neglect of it. It is therefore not superfluous to have directed our attention specifically to it in the first instance.

It has been already observed that it is the hypothesis of subdivision into infinitely small linear elements, by which alone we can apply numerical proportionals with that generality which is requisite for geometrical constructions involving the comparison of *any* given lines, which may or may not be commensurable.

But we are thus in the very rudiments necessitated to introduce methods which it is always considered desirable to exclude from the elementary portion at least of these sciences: and the whole system involves a complexity of considerations very foreign to the simplicity which distinguishes the spirit of pure geometry.

It may be true that this method admits of legitimate demonstration, but it clearly cannot be conducted except on some principle equivalent to the method of exhaustions; and which of necessity involves us in prolix and abstruse investigations. So that even if we regard the question merely as to the facilities afforded, it may be very much doubted whether to follow out such a proof with all the requisite precision, would be in itself a process at all less difficult or tedious to the learner than to go through the 5th book of Euclid.

Besides this, these methods are carefully excluded in the earlier parts of the geometrical course, and it would obviously have impaired the unity of design and completeness of the whole system (so much aimed at by the ancient geometers) to have admitted such principles in one part of the inquiry and not in another.

And further; independently of any deviation from absolute exactness, in the use of approximate values and infinite subdivision, and supposing that the ratios to be considered were all those between quantities expressed in finite and exact numbers, and that geometry had been concerned solely with quantities exactly and essentially commensurable, yet there were still stronger objections in the mind of the Greek geometer against the very first principle of such a view of the relations of magnitudes; the very idea of referring to *number* as the measure of quantity, involved in it something altogether alien from the pure conception of geometrical extension.

No such idea is ever introduced in the contemplation of those species of quantities which are the subjects of investigation in the earlier part of the elements. This is equally clear, whether we consider the comparison of lines or of surfaces: and perhaps yet more evident in regard to angles. Yet of the relations established between geometrical quantities we acquire as perfectly distinct conceptions as any which could be produced by a reference to numerical relations; e.g. we have as clear an idea of the relative magnitudes of two straight lines which may form the diagonal and side of a square, when defined by that construction, as we could do by any expression in numbers, were they capable of such mensuration. To many such relations, especially in the further parts of the system, specific names were assigned, and these constitute a sort of vocabulary of geometrical model of comparison between the magnitudes to which they apply, quite as definite and perspicuous as any expression by arithmetical measures, and wholly independent of any such.

And in like manner when we proceed to the consideration of proportionals, we still find the same total and systematic exclusion of all reference to numerical measures.

Euclid's definition of proportionals does not assume anything with respect to the nature of the quantities, but only that they are such as are capable of being considered in the relation of greater or less, and such as may have equimultiples of them taken.

Thus they may be of any species, whether such as are capable of representation by numbers or not: whether commensurable or incommensurable: but the definition does not specifically regard them with reference to any such distinctions: on the contrary, setting them aside altogether, it applies to *quantity*, considered in a far more general point of view, as including under it its various subordinate species. In a word, I conceive it clearly evident, that *Euclid's specific object in the 5th book was not to* INCLUDE INCOMMENSURABLES, *but to* EXCLUDE *all reference to the idea of* NUMBER.

But it has been alleged by the writers before referred to, and I have heard difficulties expressed on the point by eminent mathematicians, that it is impossible to form a clear idea of ratio without reference to the idea of number; and that this idea must consequently be introduced into our definition.

It appears to me that the existence of any difficulty arises from not sufficiently adverting to the nature of mathematical reasoning. I would contend at once, (paradoxical as it may be thought by some,) that it is not necessary to frame a definition conveying a distinct idea of ratio: and for this reason, that no reference is made in the subsequent reasoning to any such idea. The definition of ratio (before referred to) is never made any use of: in other words, no such definition is necessary. But in point of fact, if we wish for a definition, we have one readily supplied from that which *is* an essential part of the reasoning; viz. the definition of proportion. We might hence frame our definition of ratio by saying that "it is *that relation* which subsists between "two quantities when they are antecedent and con- "sequent to one another in a proportion according

" to the 5th definition." Much of the difficulty and misapprehension on this point (as well as in some others) apparently arises from the common mistake of supposing that definitions are intended to give clear and complete *ideas of the thing defined*, instead of being (as it appears to me they always are in mathematics) limited to *fixing the meaning of terms.* It is then no disparagement to the satisfactoriness of our definitions of ratio and proportion, that they convey no immediately tangible image to the mind: all that we are concerned with is the consideration that the reasoning shall turn wholly and strictly upon the use of the terms according to the conditions prescribed by the definition. This is in fact the case with many other definitions. For example, what idea can a beginner form of a circle from Euclid's definition? If he had not a diagram put before him to assist the conception, (and which, by the way, thus far impairs the purity of the abstract reasoning,) so far from forming a clear notion of the figure, he would most probably deny the possibility of such " a line" existing: but all this need not hinder his perception of the validity of the after reasoning; where all that he has to attend to is, that the inferences be made by virtue of the sense thus affixed to the term " circle."

It seems to be very much on the ground of an alleged want of a clear explanation of the *idea* of proportionality, that many writers have spoken of Euclid's investigation, as if it were an *unsuccessful* attempt to demonstrate the properties of proportionals on abstract principles. As I think a little consideration of the real nature of geometrical reasoning must soon remove all objection on this ground, so I am persuaded the more Euclid's method is understood, the more will it be regarded as one of the most signal triumphs of human intellect over the difficulties of an abstruse subject, which indeed are of so subtile a nature that they have eluded the grasp of some of the most skilful of modern mathematicians.

Euclid's investigation proceeds (as I believe it does in all other cases) on the sole basis of definitions of *terms:* axioms (it appears to me) being simply certain elementary theorems, in which the agreement of the subject with the predicate is perceived as soon as those terms are defined. In the present case all the properties of proportionality arise directly out of the sense affixed to the term in the 5th definition, combined in each case with that of other terms employed.

It appears to me not only useless, but absolutely injurious to the perfection of the philosophic apprehension of the subject, to attempt illustration of it by other notions of proportion, such as those derived from arithmetic, &c. which are sometimes appealed to as useful for assisting the conceptions of the learner before advancing to the investigations of the 5th book. It is to the introduction of such heterogeneous elements of thought that I ascribe most of the confusion and difficulty which has been found in the subject. If the student has been habituated to these "vulgar" notions of proportion, (as Dr. R. Simson terms them,) I should say, the sooner he can succeed in banishing them entirely from his mind the better for his correct apprehension of Euclid's method of proportionals. If it be urged that we have a natural propensity to feign to ourselves some more tangible idea of proportionals than that of the 5th definition, and more connected with their essential nature, all this I should consider merely an instance of want of due methodizing our thoughts, a fault which we should endeavour as speedily as possible to correct, by recurring strictly to the definition;—not looking for an explanation of a *thing*, but solely of a *term;* and resolutely banishing from our minds every other conception not directly involved in it.

It appears to me that it is simply a want of due attention to these considerations which occasions the difficulty found in the subject: and which is created solely by those who gratuitously involve themselves in confusion of thought by mixing up other notions of the essence of proportion with that conveyed in Euclid's definition. Let the 5th definition stand simply as a *definition of a term*, and there will be no confusion or complexity to the philosophic student, who has sufficient power of abstraction to grasp the doctrines of the 5th book. If it be alleged that the doctrine is incomprehensible to a beginner, and especially to one who is prepossessed with the notion of arithmetical proportionals, I have no objection to allow it. But the practical question, what is the easiest mode of initiating a learner into the doctrine of proportionals, so as to suffice for its application, is totally distinct from that of the philosophic completeness of Euclid's method in the 5th book.

But we may, perhaps, better obviate any difficulty, if we take another view of the subject, which will be appreciated by the analyst, by regarding (for illustration) the ratio of two quantities in the light of a particular *function* of those quantities[e]. In the first instance we know nothing of the nature or form of that function: but when we add the conditions of the fifth definition, then by degrees we develope the several characteristics and properties of such functions, as they appear in the various properties of proportionals, all investigated (as in the 5th book) without any assumption as to the nature of ratio, or the idea we can form of it: these properties constitute the idea. Indeed Euclid's method in this respect presents a remarkably close resemblance to those of the modern analysis; where the nature and character of a function at first unknown are gradually elicited: so here the Greek geometer, setting out from the very abstract function, characterized by the fifth definition, proceeds, by a singularly beautiful and refined train of reasoning, to deduce properties which successively make known the nature of the function, and enable us to apply it in subsequent investigations.

As some difficulty and objection has arisen in regard to the absence of all reference to numerical measures, it should be carefully borne in mind, that the condition of the quantities in a proportion being multiples or parts one of another, constitutes only one limited case of the general theory, and the introduction of it is no exception to the principle. *When* two quantities *are* respectively the same multiples or parts of two others, the existence of proportionality between them is proved from the *general* definition, as in props. C and D of Simson. But the student should distinctly recollect, that these propositions are not made use of for establishing any of the *general* properties of proportionals: and that the hypothesis of there being such a relation between the quantities, is not only never assumed for the demonstration of those *general* properties, but sedulously avoided by Euclid. It would have better preserved the unity of the design, if Dr. Simson had appended those theorems at the end of the book rather than have inserted them in the middle. Another exception has been alleged, arising from the adoption in the fifth definition, of the hypothesis of multiples being taken of the quantities compared; here, it is said, numerical ideas are in fact introduced, and are thus rendered essential to the investigation of proportionals.

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[e] See note E.

In reply, we have only to recur to an observation before made, and indeed prospectively to this very objection: No one who has carefully examined what has been already advanced will fail to perceive that this objection is groundless. It will entirely disappear, if in accordance with that remark, we consider that this introduction of numerical multiples does not take place between the *quantities themselves*, which are the subjects of comparison, and between which it is the design to shew that equality of ratios subsists; but it is a process *performed upon* those quantities, without affecting their own intrinsic nature, and *subsidiary* to the purpose of effecting the comparison between them. This therefore manifestly constitutes no exception whatever to the truth of the principle, that there is no introduction of the idea of numerical multiples or parts, as constituting the relation *between those quantities* which are the *actual subjects* of comparison.

Views of Professor Playfair.

Other writers, however, and those most deeply versed in the spirit and meaning of the ancient geometry, have adopted better views. Yet even here one or two instances must be noticed, in which some eminent geometers seem to have come short of a complete apprehension of the truth. Professor Playfair, in his Elements of Geometry, has followed a very different course from that adopted by the writers last alluded to. Fully aware of the peculiar excellence of Euclid's method of investigating proportionals, he makes no other alteration than that of expressing the steps of the processes by algebraic notation; thus securing a much greater degree of perspicuity, without the slightest departure from Euclid's principle. He points out, in a remarkably luminous and satisfactory manner, the relation between Euclid's definition, and that which we might derive by setting out from the common idea of proportion, which we almost always acquire in the first instance from arithmetic. It is in fact this circumstance, he justly observes, which creates one of the most usual difficulties in understanding this part of Euclid. The superiority of Euclid's method he considers to be evinced in this, that every other method (grounded on a numerical relation between the quantities themselves) must involve in the case of incommensurables a *reductio ad absurdum* [f].

This difficulty (as was pointed out by D'Alembert, who yet neglected to refer to Euclid's method) arises from the necessary introduction of the idea of infinity in comparing incommensurables. "By Euclid's method," Professor Playfair observes, "this

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[f] See note F.

"difficulty is completely overcome. It is overcome "by the introduction of the idea of indefinitude (if "I may be permitted to use the word) instead of the "idea of infinity: for *m* and *n* the multipliers em-"ployed are supposed to be indefinite, or to admit "of all possible values, and it is by the skilful use "of this condition that the necessity of indirect de-"monstrations is avoided. In the whole of geo-"metry I know not that any happier invention is "to be found." (Notes, p. 461.)

But notwithstanding his general admiration of Euclid, there is one point in which the Professor ventures to criticise him. His own object he states to be "more general than Euclid's, that is, to in-"clude the properties of proportional *numbers, as* "*well as of all magnitudes.* Euclid has not this "design, for he has given a definition of propor-"tional numbers in the 7th book very different from "that of proportional magnitudes in the 5th; and "it is not easy to justify the logic of this manner of "proceeding: for we can never speak of two num-"bers and two magnitudes, both having the same "ratios, unless the word ratio have in both cases "the same signification." (p. 462.)

In this part of the author's remarks, however, I cannot agree. It is undoubtedly true that Euclid does not specifically in the 5th book treat of numerical proportionals: but if the best reason which can be assigned to shew, that Euclid did not mean to include numerical quantity under the general notion of magnitude, as treated in the 5th book, is the fact of his having adopted another definition in the 7th book, it appears to me that the inference must fall to the ground along with the reason alleged, which we have already observed, is misconceived. Every one of those theorems in the 7th book which have theorems corresponding to them in the 5th, might have been inferred directly from those in the 5th, or rather are included in them, as particular cases. But of numerical proportionals, there are many properties essentially dependent on their numerical nature, to which there are no corresponding theorems in the 5th book; these therefore would need separate discussion. Since then a new mode of investigation must have been commenced at this point, it was of course more natural, and rendered the whole more uniform, to treat the subject of numbers on this particular principle from the outset, as a distinct case, and without reference to the more general investigations of the 5th book.

The difference in the sense of the word *ratio* in the two cases is merely the difference between the more general and the particular sense: between the genus and the species. The phrase "numbers, as well as "all magnitudes," leads me to notice the distinction which the author afterwards draws between this last term and "quantity." This term "quantity," he thinks, "ought in strictness to have been used in "the enunciation of these propositions, because we "employ the word *quantity*, to denote, not only "things extended, to which alone we give the name "of magnitude, but also numbers." (ibid.) It thus appears that he conceives the theorems of the fifth book of Euclid to apply only to "extended things," or to such magnitudes as are the subjects of comparison in geometry. I fail however, completely, in perceiving what there is in the investigation of the 5th book, which can limit our ideas to any such views of the quantities compared. Where, I would ask, in the definitions or in the conditions of the theorems of this book, is there any such limitation made of the sense in which the term *magnitude* is to be taken? On the other hand, is it not at least evident, that in whatever sense it is taken, it is such as will readily include the idea of number as well as extension? So that in fact, to speak of "numbers *as well as* magnitudes," is to speak of the species as if it were something not included in the genus: it is like naming "polygons" for example, and "hexagons" besides. In a word, it appears to me, that fully as Professor Playfair has apprehended the excellencies of Euclid's method, and ably as he has vindicated it, yet there is something more requisite to its complete elucidation: there is one step more to be taken in order to see its full compass and extent. And this step consists in getting over the confined idea of magnitude, which is not really restricted, by any thing in the 5th book, to extension, and adopting such a view of the quantities here compared, as is warranted by the tenor of Euclid's definitions, and gives the discussion the highest degree of generality.

Views of Dr. R. Simson and Dr. Robertson.

Such a view of the nature of quantity, a view including the most general conception of its nature, is, I apprehend, the real key to the method adopted by Euclid in treating of proportion; and was, I do not doubt, the view entertained by Dr. R. Simson and other geometers of that sound and rigorous school, of which he may be regarded as the head. This I conceive may be collected, both from his well known and masterly notes on the 5th book, and from other scattered passages in his writings; though it is no where explicitly stated in precise terms.

One eminent disciple of that school indeed, the late Dr. Robertson of Oxford, proposed a slight innovation on Euclid's method: but this was by no means occasioned by any difference in the fundamental conception of quantity and ratio, but arose solely out of a desire to remove the difficulties attending the apprehension of the fifth definition by learners.

That profound geometer published at Oxford in 1789 a tract, entitled "A Demonstration of the "fifth Definition of the 5th book of Euclid." As the title implies, he substitutes another definition, and regards Euclid's as properly a theorem. In the preface he observes, (in reference to the principles of proportionals,) "Most mathematicians, both "ancient and modern, have been of opinion, that "Euclid has fallen short of his usual perspicuity in "this particular. Some have questioned the truth "of the definition upon which he has founded it; "and almost all who have admitted its truth and "validity have objected to it *as a definition*. The "author of the following propositions ranks himself "amongst objectors of the last mentioned description."

He then proceeds to his own definition of proportion, and in a series of seven propositions, delivered entirely in the spirit of the ancient geometry, arrives at a deduction of Euclid's definition as a theorem.

Without entering on details, I will merely observe, that not being able myself to perceive the force of any of the objections to the 5th definition, except that of its being difficult (perhaps insuperably so) to a beginner, I cannot of course see the necessity, in a philosophical point of view, for the introduction of this somewhat prolix, though acute and skilful investigation. Still less (in a practical light) can I imagine it likely to diminish the difficulties of a learner.

Nature of Abstract Quantity.

Let us then recur to our conception of quantity in its most general sense; a conception, which, once understood and adopted, appears to me to explain, as well the difficulties of the subject, as the origin of the misconceptions which have prevailed respecting it. Our idea of *quantity in general* is very easily understood, if we simply consider it in the same light as other *general ideas;* and only bear in mind, and apply to this case, the same process of abstraction by which we arrive at those ideas in other cases.

We form ideas of so much in length, so much in area, so much in solidity, so much in duration, so much in number, so much in intensity, &c.

From all these particular ideas we abstract what belongs to the particular nature of length, of surface, of capacity, of duration, of number, &c. and thus form our abstract idea of " SO MUCH," or of QUANTITY IN GENERAL: a comprehensive generic term, including under it all the particular species of quantity[g].

In the first part of geometry, i. e. in the first four books of the elements, we consider *quantity of ex-*

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[g] See note G.

tension, and its properties in regard to linear and superficial space, as referring to certain geometrical figures. And it is essential to observe, that the estimate we form of these magnitudes consists in simply considering them as *so much length, or so much surface*, without assuming any particular scale or standard of measurement: we compare such lines and surfaces together, and establish relations of equality between them, as constituted under certain given conditions: and these form certain relations which are strictly and properly *geometrical*, as not being determined with reference to any *numerical* measurement.

We proceed in like manner in the 5th book to investigate certain more varied and comprehensive relations, which subsist between quantities considered all along in the most general and abstract point of view, and which give the properties of proportionals. Nothing is introduced which refers to any of those particular characteristics, by which the different species of quantity are distinguished; the conclusions, therefore, are absolutely general, and may subsequently be applied indifferently to any of the particular species. This is accordingly done in the 6th book, with regard to linear and superficial extension, and in the 11th and 12th to solid extension. The nature of the subject treated in the 7th and other books requires a different and more restricted mode of investigation, as applying to properties peculiar to one species of quantity; viz. number: and to certain highly curious relations which subsist when number is employed to measure extension[h].

[h] See note II.

But the discussion of these properties, which arise wholly out of the numerical system of measurement adopted, is studiously kept entirely distinct from the purely geometrical investigations. Those of Euclid's books, in which the numerical principle is introduced, may be taken as an entirely distinct arithmetical treatise; and, there can be little doubt, originally did actually form a separate work. There is never the least intermixture of the methods, nor is anything in the geometrical books in the smallest degree dependent upon these others. Here all such ideas are totally excluded. In fact, to investigate the theorems of geometry by first tracing the properties of number, then applying numerical measures to the geometrical magnitudes, and thus establishing their relations by means of those of the numbers which measured them, was a device of too gross and mechanical a nature to suit the refined and fastidious spirit of the Greek geometry: the lofty genius of its speculations could condescend to nothing less than a pure contemplation of the abstraction of *quantity* itself: and of quantity of extension in any of its dimensions, as such, simply, without the intervention of any arithmetical medium of comparison.

Compound Ratio.

For completing the illustration of our subject it will be necessary to advert briefly to the nature of *compound ratios*[i].

The editors of Euclid, before Dr. Simson had retained a definition of compound ratios, confounding them with multiplication, inserted by Theon, in the

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[i] See note I.

6th book, instead of the simple and correct one now given in its proper place, in the 5th, by Dr. Simson, as a restoration of what was manifestly Euclid's real method. And the confusion of ideas which different commentators have felt on this point is betrayed in the vague language by which they appear trying to evade the necessity of confessing that their notion is limited to numerical multiplication. Nothing can be more masterly than Dr. Simson's exposure of them in his note on the 23d prop. of the 6th book.

His own account of the real nature and use of compound ratio (which he was perhaps the first, among modern writers, to point out) is equally simple and satisfactory. "Every proposition," he observes, "in which it is made use of, may without "it be both enunciated and demonstrated." The use of it then, he points out, is solely to avoid circumlocution. "The ancient geometers, when they observed "that the enunciation could be made shorter *by* "*giving a name* to the ratio which the first quan-"tity has to the last, by which name the interme-"diate ratios might likewise be signified . . . . . they "*called* this ratio of the first to the last, the ratio "compounded of the 1st to the 2d, and of the 2d to "the 3d," &c.

Between the composition of ratios, as established in the 5th book of Euclid, and the rectangles under proportional sides, and again the numerical multiplication of fractions, there is unquestionably an analogy observable; and there has been a strong propensity among mathematicians, who did not take the trouble of a very scrupulous examination of first principles, to regard it not only as an analogy, but as something approaching to an identity between these several cases. If number be the essence of all ratios, then indeed this is evident; but if not, there is not only a want of proof, but the assertion is absolutely untrue.

The writers who proceed on the numerical principle, though they retain some apparent distinction in terms, yet of course can recognise no real difference. If ratio essentially involve the idea of number, in using multiplication of the numbers for composition of ratios, there is absolutely no difference, and no substitution of one method for another; they are one and the same. Nor when this process is introduced, can the investigation be called in any degree more properly algebraical than without it. Any comparison which can be made between the methods is a mere question of style.

Those again who admit Euclid's system to be something different from a numerical investigation, and composition of ratios, in particular, something different from multiplication, yet finding that, for the facility of investigation, it is necessary to have some more ready and comprehensive method, they can only make such a substitution on legitimate grounds, by first assuming the system of numerical measures, exact, or approximate, if not as universal, yet as applying to all those kinds of quantity which they have occasion to consider. This hypothesis made, they can proceed directly by multiplication to all those cases, where, according to Euclid's method, composition of ratios would be necessary. Proceeding in this way, however, we should soon find ourselves beyond the limits of those results to which any theorems of the composition of ratios, as laid down by Euclid, could conduct us.

Yet if the two principles are coextensive, and the one is merely an easier mode of expressing the other, adopted for convenience, with perhaps some sacrifice in point of rigid precision, we ought at least always to be able to satisfy ourselves of the correctness of the more simplified process, by resolving it into an application of the more rigorous principle. We ought always at least to have it in our power to recur to the ancient method, and to reduce the multiplication into a composition of ratios. But if we proceed to almost any analytical processes involving multiplication, in attempting this we shall find ourselves stopped at the very outset; as soon, that is, as we encounter an equation between the product of the extremes and means of a proportion[k].

If we take the subject in the unlimited sense in which Euclid has treated it, it is then evident, beyond the possibility of question, that the composition of ratios, according to the idea of Euclid, is in no degree to be identified with multiplication; there is no proof even of a similarity, except this, that in the particular case where the terms of the ratios are quantities expressed by numbers, then it is seen that the composition of the ratios is effected by multiplication of the numbers. But in the most general case, when the quantities are not necessarily numbers, and where all reference to that more particular case is of course excluded, it is certainly not apparent, nor has it, as far as I am aware, been demonstrated, that any thing resembling multiplication takes place in the composition of ratios. We have indeed, in another particular case, (that of the areas of rectangles whose sides are reciprocally proportional,) a close and evident analogy with a numerical product: but this analogy is only established by supposing numerical values of the sides taken and multiplied together. And in rectangles there are obvious analogies in the same way with the other properties of products.

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[k] See note K.

In elementary algebra, mere addition and subtraction is not limited to any particular species of quantity; nor does it of necessity suppose the quantities concerned to be numbers. But, in the common systems, as soon as we come to introduce the idea of multiplication, there, by the very definition, we of necessity limit our consideration to quantities, one at least of which is a *number*, and usually both. This refers to algebraical proportionals, and to all their applications, that is, to the whole superstructure of modern mathematical science.

The doctrine of numerical proportionals, as established in common algebra, comprises corresponding particular cases of all the properties delivered by Euclid of quantities in general. The investigation of course gains in brevity what it wants in generality. But it extends to a greater number of properties: and is principally distinguished in that it embraces the equality of the products of the extremes and means of a proportion. This, with others dependent on it, since they involve the idea of multiplication, of course have no place in Euclid's system. The analogous property of rectangles (established in the 6th book) does not supply the deficiency, because it applies only to one species of quantity, viz. superficial area, and not to any thing equivalent or analogous to a *product* between abstract quantities.

But in the general view of the subject, though, as we have observed, no investigation has hitherto been proposed to establish an extended analogy of relations and properties between the composition of ratios as referring universally to quantity in the abstract, and those results dependent on multiplication, which are as yet restricted essentially to quantities as measured by number, it becomes a question of the highest interest to examine whether any such analogies can be legitimately established. Such an investigation must refer only to our notion of the nature of quantity contemplated in the abstract; and to the simple view of the composition of ratios, adopted in the 5th book of Euclid. Upon these grounds the investigation must essentially proceed; and the exclusion of all numerical multiplication is the characteristic condition. The demonstration of such general properties has, I apprehend, not been considered by mathematicians only because they have not been led to recognise its importance.

If then the truth of the preceding views be allowed, I trust they may eventually supply the groundwork for the prosecution of an inquiry so essential to the establishment of a comprehensive and universal analysis.

Conclusion.

Of the preceding discussion, I am prepared to expect that various opinions will be entertained. Some may question the soundness of the reasoning, or the correctness of the principle. Others may think it altogether superfluous, and of too speculative and abstract a nature to be of any real use in the study of mathematics; and may possibly regard it as little more than an over refined distinction in terms, and undeserving of attention, since it cannot be intelligibly addressed to a beginner, and is superfluous to the advanced student.

Should such objections arise, I merely beg to say, that the subject is proposed for examination in no other than a purely speculative point of view; and the importance of such a view of it must be determined by the opinion which may be formed of the reasoning on which the conclusions are supported. I have therefore only to hope, that some readers may be found so far interested in the subject as to think it worth while to give the whole a fair examination, and make known their views of it.

NOTES.

Note A.

Dr. I. Barrow, in his "Lectiones, &c. anno 1666," [ed. Lond. 1684,] enters upon the subject of proportionals with the highest commendations of its importance: "Nunc ad illam matheseos velut animam pene deventum est, proportionalitatem: e qua fere pendet quicquid uspiam in mathematicis mirabile vel abstrusum demonstratur." (p. 167.) And in another passage, speaking again of proportions, says, "Quæ scilicet anima matheseos et nucleus habetur, imo disciplinarum omnium vinculum (*δεσμος των μαθηματων*) a Platone (in Timæo) dicitur." (p. 274.)

The author's view of proportion is perhaps most briefly and clearly stated in the 6th lecture, (1st series, p. 114.) ...... "ratio vel proportio; quæ fere nihil est aliud quam ex quantorum comparatione resultans æqualitatis aut inæqualitatis determinatio quædam." The nature of this *determination* does not very clearly appear, but he afterwards says that such determination "numeris vel aliis idoneis terminis expressa, dici solet ipsorum proportio." Here we are brought somewhat nearer to a precise idea: he seems to consider the determination of the ratio to be effected by *numbers:* for the "aliis idoneis terminis" is an expression to which, in consistency with what he says elsewhere, we can hardly suppose him to attach any other meaning than that of symbols expressive of such numerical measures. The elaborate citation of authorities which he produces from the ancient philosophers as to the meaning of the term *μετρον*, throws little real light on the subject. It is much more to our purpose to find that in the 4th lecture of the 2d series he gives the definition before cited.

In this 2d series the author enters upon a variety of obscure questions which had arisen relative to the nature of ratios; and which exhibit little more than one continued tissue of those perplexities of verbal mysticism which in that age were so commonly mistaken for metaphysical reasoning.

In the 3d lecture he gives an elaborate vindication of the definition of "ratio" inserted in the 5th book. According to this definition, ratio is a relation between magnitudes "κατα πηλικοτητα;" which he thus interprets; "Quoad "quantitatem; hoc est quoad magnitudinis suæ deter-"minationem, vel magnitudinem ipsam determinatam." (page 220.) And again (ibid.), "Non admodum probo "quod κατα πηλικοτητα nonnulli exponunt secundum quod "unum alterius tantuplum est, vel secundum tantuplicita-"tem," &c. These phrases he turns into ridicule. But it is extremely difficult to make out why he should disapprove of such interpretations, since they agree closely with the numerical view of the subject which he adopts.

In his 5th lecture the author enters upon his critique of the views of Tacquet, Borelli, and Gregory Vincent. He pursues the discussion to the subject of compound ratios: not however with that clearness which could be wished.

In the 6th lecture he expressly represents Euclid's 5th definition as framed with the view of including both commensurables and incommensurables. (p. 280.) And the 7th and 8th are devoted to the refutation of the various objections which had been brought against this definition. In his refutation of these various objections the author appears more successful than in the preceding portion of his discussions.

It is remarkable, that in his own edition of Euclid all the other definitions of the 5th book, except the 5th, are accompanied with illustrations by means of numerical fractions. And in his comment he formally defines "the *quantity* of "a ratio as known from *dividing* the antecedent by the "consequent."

The same numerical view of the ratios of quantities was adopted by Dr. Wallis: (Arith. p. 226.) "Rationes omnes "geometricas quarumcunque ad invicem quantitatum esse "inter se homogeneas, quippe in genere numeroso omnes. "......Ubi comparatio fit quoad rationem, quæ emergit ra-"tio comparatorum, genus non raro deserit, et transit in "genus numerosum, cujuscunque sint generis quæ compa-"rantur."

Dr. Barrow censures this, as if it were something different from his own view of the numerical nature of ratio: the difference is not easily apparent:—"quia nullum ego vel "rationum vel numerorum genus sub quantitate comprehen-"sum agnosco." (p. 206. 2nd Series, Lect. ii. 1666.)

Note B.

I here allude particularly to a small anonymous tract on Proportion, ascribed, I believe, to that highly distinguished analyst, sir J. Ivory.

In his introductory remarks, the author observes, "Eu-"clid has discovered great penetration and skill in his mode "of treating proportion; his definition is perfectly general, "comprehending quantities that are not, as well as those "that are, commensurable. It is also easily applied in the "course of investigation. In these essential advantages, "the view of proportion, which we owe to the Greek geo-"meter, is preferable to any other that has yet appeared, "and there is no reason to think that it will ever be sur-"passed." On the other hand, he states the difficulties attending the study of it, and its unsuitableness to the beginner. In his definitions he uses the terms *number* and *magnitude* as synonymous.

Speaking of incommensurables, he says, "In this respect "there is a defect in the order, at least, of Euclid's elements. "For the learner can neither understand the full force of "the fifth definition of the fifth book, nor comprehend "what could induce the author to adopt so artificial a cri-"terion of proportion, without a knowledge of incommen-"surables, which subject is nevertheless not touched upon "till the 10th book."

Note C.

Sir J. Leslie, at the commencement of the 5th book of his geometry, (vol. i. p. 125,) observes, that his former books (corresponding nearly to the first 4 of Euclid) "treat of "magnitude as *concrete*."......"By introducing the idea of "*number* into geometry, a new scene is opened, and a far "wider prospect rises into view; magnitude being con-"sidered as *discrete*, or composed of integrant parts, be-"comes assimilated to *multitude*," &c. He then explains the measurement of mathematical quantities by the introduction of this idea, that is, by supposing them represented by numbers, and goes on to discuss the nature of incommensurables, and to apply the doctrine of proportionals to them, by means of approximation to any degree of accuracy. And, by applying these methods, he distinctly contends, that at this point, "geometry is brought under the "dominion of arithmetic."

Geometry (it appears to me) if at all, must be said to be "brought under the dominion of arithmetic," at a much earlier period of Euclid's course than the 6th book, viz. in such instances as the Corrollaries of i. 32. and in iv. 16, where the demonstration essentially turns on the *arithmetical theorem*, by no means at once evident, that the difference of the arcs, subtended by the sides of the equilateral triangle and pentagon contains $\frac{2}{15}$ of the whole circumference.

As to the critical meaning of the term *κατα πηλικοτητα* discussed by this author, I am quite unable to perceive, on any philological grounds, why it should be restricted to *numerical* magnitude, more than to any other species.

The author's other views referred to, occur in the notes (vol. i. p. 333.) and are stated in the following passage.

"The great object which the framer of the elements had "proposed to himself, by adopting such an artificial defi-"nition of proportion, was to obviate the difficulties arising "from the consideration of incommensurable quantities. "Under the shelter of a certain indefinitude of principle, "he has contrived rather to evade those difficulties than

" fairly to meet them. Euclid seems not indeed to grasp " the subject with a steady and comprehensive hold. In " his seventh book, which treats of the properties of number, " he abandons his former definition of proportion, for another that is more natural, though imperfectly developed. " Through the whole contexture of the elements, we may " discern the influence of that mysticism, which prevailed " in the Platonic school. The language sometimes used in " the fifth book would imply, that ratios are not mere conceptions of the mind, but have a real and substantial essence.

" The obscurity that confessedly pervades the fifth book " of Euclid, being thus occasioned solely by the attempt to " extend the definition of proportion to the case of incommensurables, the theory of which is contained in his tenth book, " the pertinacity of modern editors of the Elements, in retaining such an intricate definition, appears the more singular, since, omitting all the books relating to the properties " of numbers, they have not given the slightest intimation " respecting even the existence of incommensurable quantities.

" The notion of proportionality involves in it necessarily " the idea of *number*. The doctrine of proportionals " hence constitutes a branch of universal arithmetic."

It is unfortunate, that the author did not adduce some instances of the alleged *realism* of the 5th book.

In the severe critique on this work, which appeared in the Edinb. Review, N°. XXXIX, (p. 93.) the subject of proportion is particularly referred to. Objecting, however, as he does, to Leslie's view of the subject, the critic does not appear to me much to improve upon it in the definition he proposes to substitute; which still goes on the arithmetical principle: he adheres to the opinion, that Euclid's object was to include incommensurables, and concurs in Leslie's censure of modern editors, for omitting the 10th book. A review of the same work in the Quarterly, N°. VII, throws no new light on the subject.

Note D.

Professor Playfair does not conceive there is any other reason for supposing the definition of ratio to be spurious, except its being useless. He considers it probable, that Euclid might have introduced it to give a first general notion. (Geometry, notes, p. 457.)

Now, if the definition of ratio be allowed to remain in the text, it is admitted, that the ordinary translation gives a sentence which conveys little meaning, and at all events is of no use, being never subsequently referred to. We might be well satisfied to acquiesce in the opinion of its spuriousness, on such authority as the critical judgment of Dr. R. Simson. If, however, we adopt this numerical interpretation, it appears to me simply to furnish a still stronger argument, from the internal evidence of its utter inconsistency with Euclid's method, that this definition could not have been his, or at least could not have been inserted by him in this place.

Note E.

The rudiment of the idea which I have expressed by describing the terms of a compound ratio, as functions respectively of the original antecedents and consequents, may perhaps be traced in what Professor Playfair has said on the same subject in his notes. (p. 463.)

" An objection which naturally occurs to the use of the
" term compound ratio, arises from its not being evident
" how the ratios described in the definition, determine in
" any way the ratio which they are said to compound, since
" the magnitudes compounding them are assumed at plea-
" sure. It may be of use for removing this difficulty to
" state the matter as follows:".. ." the first of the ante-
" cedents has, to the last of the consequents, a ratio which
" evidently depends on the intermediate ratios; because if
" they are determined it is determined also. This depend-
" ance of the one ratio on all the other ratios, is expressed
" by saying that it is compounded of them....the exact

“ nature of this dependance, and how the one thing is de-
“ termined by the other, it is not the business of the defi-
“ nition to explain, but merely to give a name to a rela-
“ tion,” &c.

Note F.

The acute author of the treatise on the study of mathematics in the Library of Useful Knowledge, has largely discussed the subject of our elementary ideas of proportion; and particularly the difference between the common algebraic definition and that given by Euclid, (p.83, &c.) He has clearly shewn the inadequacy of the common definition, if we would treat the subject with that generality which is indispensable; and the necessity for one which shall include incommensurables. And having given one so framed, he has shewn its identity with the common definition, and that Euclid's is a consequence from it. This definition, he observes, and the methods by which its identity with that in common use is established, bear a close analogy to the process used by the ancients, and denominated by the moderns the method of exhaustions. This circumstance, however, would probably in the opinion of most teachers, at once exclude such a method from *elementary* mathematics. Yet a mode of proceeding, virtually the same, is certainly indispensable for the satisfactory generalization of algebra, and for its applications to geometry, if the *numerical* principle be assumed as the basis of the whole. The deduction of Euclid's definition is of course limited by the same conditions as the original hypothesis; and is only so deduced, understood as referring to numerical proportionals.

Our question, however, is rather concerned with the legitimacy of that principle of *numerical measure* itself.

“ On ne peut démontrer (says D'Alembert) que de cette
“ manière, (la reduction à l'absurde) la plupart des proposi-
“ tions qui regardent les incommensurables. L'idée de
“ l'infini entre au moins implicitement dans la notion de ces
“ sortes de quantités; et comme nous n'avons qu'une idée
“ negative de l'infini, on ne peut démontrer directement et

a priori tout ce qui concerne l'infini mathématique." (Encyl. art. Géométrie.)

In every application of algebraic proportionals to a geometrical construction, we must first adopt the supposition, *that the lines may be estimated in numbers:* and in the whole system of loci, we must assume that the coordinates are measured according to some numerical scale, and that their symbols represent those numerical measures before the relation expressed by the algebraical equation can be applied to them. And not only this, but from the considerations before adduced, we are necessitated to conceive this numerical division carried on *ad infinitum*, and the lines to be composed of infinitely small elements. Thus, according to the ordinary system, not a single step can be legitimately taken, even in the very first rudiments of algebraic geometry without infinitesimals.

Vague prejudices have often been entertained against the introduction of algebra into geometry. Yet the specific grounds of objection have never been very clearly alleged; but here, if anywhere, we find some ground for such objections. Infinite subdivision, and the substitution of approximate for exact ratios, are surely principles which the rigour of pure geometry might fairly refuse to allow.

The student may consult a paper by the late Professor Woodhouse, on the independance of algebra and geometry, in the Philos. Trans. 1802, p. 85.

Note G.

That some theorists, adopting the abstract notion of quantity, should have framed unsound and empty speculations upon it, is of course no argument against the primary idea itself. Hobbes, for example, entertained probably a clear notion of quantity in the abstract, though he has run into some idle distinctions upon it: thus he observes, (Dial. 3. p. 81.) " Quantitas cujuscunque rei quantitati in " abstracto cujuslibet alterius rei homogenea est, ideoque " linearum, superficiorum solidorum, temporis, motus, vis, " ponderis, roboris, resistentiæ quantitates sunt homogeneæ, "etsi res ipsæ sunt heterogeneæ." (p. 77.) "Video jam "quanquam absurdum sit lineam dicere tempori æqualem "esse, non tamen absurde dici quantitatem lineæ æqualem "esse temporis quantitati."

Barrow controverts many of Hobbes's views connected with the subject, which in fact turn entirely upon mere verbal disputes: (Lect. i. 2nd Series, p. 179. et seq.) He ridicules the idea of abstract quantity, where he ought to have confined his censure to the inferences which Hobbes has made from it. But even these inferences, though of a frivolous character, by no means involve any thing absolutely absurd or untrue. Abstract quantities can neither be said to be homogeneous nor heterogeneous. If we take any particular *species* of quantity, as numbers, or lines, &c. to represent the magnitudes in question, then it may be perfectly true, that ratios may subsist between those quantities, though not between the magnitudes or things which they measure.

Dr. Reid in his Essay on Quantity (Phil. Trans. 1748.) defines mathematics to be "the doctrine of measure." And subsequently says, "the object of this science is commonly "said to be *quantity*; in which case quantity ought to be "defined, *what may be measured.* Those who have de- "fined quantity to be whatever is capable of more or less, "have given too wide a notion of it, which has led some "persons to apply mathematical reasoning to subjects that "do not admit of it." He hence infers, that the proper object of this science is not merely what may be increased or diminished, but what may be *multiplied and divided.*

The erroneous applications to which he refers are supposed by D. Stewart to be those of Dr. Hutcheson, who went so far as to express algebraically the merit and demerit of actions, &c. (Philos. of the Mind, vol. ii. notes, p. 539.)

In the whole of this paper, the author seems to me rather to assume than prove, or even attempt to prove, that we can form no idea of *quantity* without *number*, and to assume, absolutely at variance with fact, that we can form no idea of *ratio* without *multiplication*. This last notion is obviously refuted by the fact, that Euclid has legitimately deduced the doctrine of ratios in the 5th book without any such assumption.

Dr. Reid has in the same paper drawn a distinction between proper and improper quantities. The former being such as are measured by those of their own kind, as a line by a line, &c. which he contends are all measured by numbers. The latter, such as can be subjected to measurement only, by the application of the former; as for example, velocity measured by the space or line traversed in a given time; &c. He enumerates several instances of each kind; of the first, " extension, duration, number, and proportion :" of the latter, " velocity, density, forces, and the different " orders of fluxions." In all this there appears to me no small degree of confusion of ideas evinced. Indeed D. Stewart considers this enumeration of proper quantities especially to be open to great objection (*ubi supra*). The slightest consideration might serve to shew, that *proportion* can on no hypothesis have any claim to be considered as a *species of quantity:* and the author is absolutely inconsistent with himself, in reckoning number as a *species* of quantity, when he all along makes it *identical* with the notion of quantity.

M. Schröder in his prolegomena, the first part of his " Elementa Matheseos puræ," (Traj. ad Rhen. 1831.) has entered into a discussion of the principles and methods of mathematical reasoning of voluminous extent. In reference to our present subject, this author, after giving some definitions of the terms quantity, &c. and the distinctions between geometry and algebra, proceeds to explain his views of the measure of quantity, and here seems to consider number and a unit as essential: in incommensurables he says, " the unit must be *infinitely small*." (§. 7.)

Also (§. 10.) that number applied as a measure to lines, surfaces, " et ad alia quanta...proprie non est quantum sed " potius ratio quæ intercedit inter hæc quanta atque eo- " rum unitates." He then designates numbers as " *quanta* "*arithmetica*" as distinguished from "quanta geometrica."

In §. 15. he refers to the distinctions which have been drawn between "quantum continens, concretum vel continuum," and "quantum discretum," but nothing is elicited which tends much to the real elucidation of the subject. He speaks of "quantum extensum," or geometrical magnitude, whence we have "quantitas extensiva:" and from this he proceeds to another species, sometimes considered "quantitas intensiva," and ascribed to degrees of force, weight, &c. &c. to which, he justly observes, mathematical considerations can only be applied by the introduction of "some quantitas extensiva," to which they may be referred.

Note H.

Some eminent mathematicians have sought to refute the objection, arising from the alleged inversion in the logical order of the 5th and 7th books, by a reference to the doubts which have been entertained on critical grounds, whether the present order of the books of the elements is Euclid's original arrangement. Professor Flauti of Naples has argued with considerable ability, that the 7th, 8th, 9th and 10th books had formed a separate work; and that the reference made to them, as well as to the other books, in the 13th, 14th and 15th, merely supposes that both works were in the hands of students.

The two last books, however, are now generally allowed not to be Euclid's: they are expressly ascribed to Hypsicles, by the Arabian translator of the first thirteen. As far as internal evidence can guide us, it seems to me a far more probable supposition, that the 13th book also belonged originally to this separate *numerical* or *algebraical* treatise, rather than to the elements of pure geometry, which are comprised in the first six books, together with those now reckoned as the 11th and 12th, from which all idea of number was excluded. I can see nothing whatever in any argument drawn from the reference made in one book to the others; which might equally well be made from one distinct work to another. Prof. Flauti's edition of Euclid is comprised in the first two volumes of his Corso di Geometria.

Note I.

It cannot be necessary formally to refer to Dr. R. Simson as the most luminous and masterly expositor of almost every point connected with the spirit and genius of the ancient geometry, especially of the doctrine of ratios.

It is, however, much to be regretted, that the particular question treated in the preceding pages was not more specifically and fully discussed by that profound geometer, than whom perhaps there never has existed an individual more peculiarly qualified for the task.

In reference to what is called (though improperly) the logic of mathematics, his great superiority even over those who much excelled him in some other respects, appears fully substantiated, as well from the whole tenor of his labours as from what he has written incidentally bearing upon this particular question; Dr. Moor emphatically wrote under his portrait, " Geometriam sub tyranno barbaro, sæva servi-
" tute diu squalentem, in libertatem et decus antiquum vin-
" dicavit unus." But perhaps the full force of this testimony (so far as Euclid is concerned) has not been generally appreciated in all the extent to which it may apply, and may perhaps have been rendered more striking in the course of these remarks.

There is, indeed, an expression, in the preface to his conic sections, from which we might have been led to imagine that he contemplated producing a dissertation on the essential distinction between algebra and geometry, which is in fact involved in the subject here discussed: but in one of his letters, given in his Life, by Dr. Traill, (p. 121,) he distinctly states, that though an impression to this effect had gone abroad, yet his meaning had been mistaken, and that he never entertained such a design.

In his mathematical correspondence in the Life alluded to, we might naturally expect to find at least something bearing directly on our present question; very little, however, appears; one such passage is the following: his friend and pupil, Mr. Scott, had remarked in a letter to him, "The 5th book is not peculiar to geometry, but is equally "applicable to quantity as it is to magnitude: nor does the "doctrine of proportion receive any evidence from its being "expounded by lines—the symbols of algebra would, I "think, be of use here." &c. (p. 115.) To this Dr. Simson replies, "As to the 5th book, I do not see that the demon-"strations in it can receive any help from algebra: and the "straight lines made use of in it make the demonstrations "clearer and easier than they would be without them. I "do not understand your meaning when you say, any ana-"logy may be called algebraical as well as geometrical. "The expressing lines by a single letter does not make "analogy or any thing else algebraical, any more than "when they are expressed by the two letters at their ends; "nor do I think anything can be called algebraical where "no operation peculiar to algebra is made use of. I should "be glad to see an algebraical demonstration of some pro-"position in the 5th book, e. g. the 17th." &c. (p. 121.)

Nothing can be more sound or just than these remarks: but still the characteristic point of distinction between the two methods is not explicitly pointed out: and the question, *What operations are peculiar* to algebra in reference to *proportionals?* yet remains to be answered: *Such as depend on the* NUMERICAL *measure of ratios* is my reply; and I am convinced would have been Dr. Simson's.

"Indeed," observes Prof. Playfair, "to deliver investi-"gation from those modes of expression which involve the "*composition of ratios*, and to substitute in their room the "*multiplication of the numerical measures*, is of itself a "very great advantage arising from the introduction of al-"gebra into geometry." Again, speaking of the substitution of the arithmetical operations of multiplication and division, for the geometrical method of the composition and resolution of ratios, says, "Of the 1st of these, the idea is "so clear, and the work so simple; of the 2nd, the idea is "comparatively so obscure, and the process so complex,

" that the substitution of the former for the latter could not " but be accompanied with great advantage. This is, in- " deed, what constitutes the great difference in *practice*, be- " tween the algebraic and the geometric method of treating " quantity."........." the advantage is found especially in " complex cases, and in its applicability to all sorts of quan- " tities, whether constant or variable, finite or infinite,"...... " few of the early algebraists seem to have been aware of " these advantages." (Historical Diss. p. 44.)

The advantage, and saving of trouble, is no doubt very great: but it is purchased simply at the expense of demonstration. The idea of composition of ratios is only obscure, because mathematicians gratuitously perplex themselves with considerations not properly belonging to it.

Dr. Traill (in continuation of a passage before quoted) appears to me to come very near upon the truth, without actually explaining it, when he says;—" An analysis or " demonstration, in which many combinations of ratios are " employed, may generally indeed be much shortened, by " admitting *multiplication*, division, or other operations, " *peculiar* to arithmetic or algebra; but the investigation or " demonstration *then becomes truly algebraical*." (p. 124.)

The question may be illustrated from an expression used by Dr. Davies Gilbert, in his paper on Negative and Imaginary Quantities, &c. (Phil. Trans. 1831. p. 92.)

" In common language and for ordinary purposes, multi- " plication is considered as an abbreviated addition: but it " would be a superfluous waste of time to demonstrate be- " fore this society, that multiplication is always an affair " of ratios."

This, indeed, will readily be admitted, multiplication being involved in the very definition of ratio, as applied to numbers. It is precisely the *converse* of this, that the modern geometricians seem so anxious to assume; viz. that ratio is always an affair of multiplication. This, I apprehend, is the very idea which the ancients so scrupulously shunned, and to avoid introducing which, they adopted such apparently circuitous methods.

Note K.

Des Cartes, at the commencement of his geometry, thus explains the analogy between the operations of arithmetic and those of geometry:

"In geometria, quod spectat ad lineas, quæ queruntur "præparandas, ut cognita fiant, aliud faciendum non est, "quam ut vel ipsis adduntur, vel ab iisdem subtrahantur "aliæ: vel etiam in una sit, (quæ vocetur unitas, ut eo "commodius ad numeros referatur, quamque communiter "pro libitu assumere licet,) atque præter hanc adhuc aliæ "duæ, ut ad ipsas inveniatur quarta, quæ sit ad alterutram "ut est altera ad unitatem, quod idem est atque multiplicatio; vel ut per ipsas inveniatur quarta quæ sit ad unam "ex illis duabus ut unitas ad alteram, quod convenit cum "divisione; vel denique ut inter unitatem aliam quandam "rectam inveniantur, una, aut duæ, pluresve mediæ proportionales, quod idem est, quod radicis quadratæ aut cubicæ, &c. extractio: neque enim hosce arithmetices terminos, ut facilius intelligi possim, in geometriam introducere verebor." (Cartesii Geometria, ed. Schooten. Amst. 1659.)

He then explains the geometrical representation of multiplication and division, by a construction, thus: placing the lines (a, b) which are the quantities to be multiplied at any angle, and unity being set off on one of them, as on (a,) and its extremity joined with the extremity of (b,) then through the extremity of (a) a line parallel to this will cut off on (b) produced a line (c) which represents the product.

And conversely, the quotient of (c) divided by (a) is given by the same construction, and expressed by (b.)

The square root is found by the ordinate of a circle, one segment of the diameter being unity; and in the sequel, the author proceeds to geometrical constructions for the roots of the higher equations.

But the above method, as regards multiplication, evidently assumes the equating of extreme and mean products, and consequently involves the idea of number: hence, though the very same construction is adopted by Newton, he correctly introduces it only in the treatise which he appropriately and distinctively entitles, *ARITHMETICA UNIVERSALIS.* (p. 7.)

Every reader of the Principia, of course, remarks Newton's close adherence to the ancient geometry, and is probably struck with the phraseology which he is thus led to use; and which might be thought a mere peculiarity of style, in some instances not only affectedly singular, but absolutely inconvenient. Thus the expressions "ductum in" and "applicatum ad," instead of multiplication and division, and the "rectangle" and "solid," always used to designate quantities of two and three dimensions, might, perhaps, appear mere formalities: but in this, Newton doubtless saw clearly the distinction he was keeping up between those relations of quantities which are *purely geometrical*, and those which are distinguished from them by the introduction of the idea of *number:* he takes given straight lines to represent the quantities he has to consider, without any supposition of their being measured or represented by numbers; he consequently represents the ratio compounded of the ratios of such *lines*, by taking the rectangles; the idea of numerical multiplication is never introduced: the *symbol* is indeed employed for brevity, to express this composition of ratios; but there is no where any confusion of the two ideas.

It is true, indeed, that Newton has not clearly specified this distinction in a passage where perhaps we might have expected it, viz. in the scholium to the 10th lemma, where he is explaining the sense in which he speaks of one quantity varying as another; but here, if we bear in mind his language in other places, we cannot mistake the distinction:

"Et si earum aliqua dicatur esse ut sunt aliæ duæ vel "plures directe vel inverse: sensus est quod prima augetur "vel diminuitur in ratione quæ componitur ex rationibus "in quibus aliæ vel aliarum reciprocæ augentur vel diminuuntur. Ut si *A* dicatur esse ut *B* directe et *C* directe "et *D* inverse: sensus est quod *A* augetur vel diminuitur

" in eadem ratione cum $B \times C \times \frac{1}{D}$ hoc est, quod A et $\frac{BC}{D}$ " sunt ad invicem in ratione data."

This last illustration sufficiently shews (in conformity with his invariable method) that the symbol of multiplication is here to be understood in no other sense than as expressing the solid *BC* applied to the line *D*.

Note L.

Since this essay was completed, I have seen Mr. Whewell's able pamphlet on " the Study of Mathematics," &c.; in the course of which he introduces remarks bearing in some degree on the subject here discussed ; perhaps indeed, strictly speaking, less closely in reality than in appearance : since the object of the author's argument throughout is rather of a practical character ; being directed to the question of the best topics and the best methods of elementary instruction ; whereas, my inquiry is wholly remote from such questions, and refers solely to more abstract principles. If then, in the discussion of such practical questions, Mr. Whewell introduces some views of the nature of proportionals and of Euclid's system, which may be apparently contradicted by what has been here urged on the subject, it should be borne in mind how totally different are the grounds on which we respectively take our stand, and the objects to which our arguments are respectively directed.

On this plea I shall feel excused from entering upon particular references to the arguments of that pamphlet. Those readers who may admit the justice of what I have advanced in my essay, will best judge how far it applies to any of the topics of the pamphlet.

NOTES

ON THE

INDICA OF CTESIAS.

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NOTES

ON THE

INDICA OF CTESIAS.

READ TO THE ASHMOLEAN SOCIETY, FEB. 5, 1836.

IN offering to the Ashmolean Society some observations on the statements concerning India which were compiled four centuries before our era by Ctesias, the Greek physician of Artaxerxes Mnemon, it must be unnecessary to preface my remarks by any account of that author or his writings. These are subjects with which all the members are no doubt familiar; and they will also be well aware how low a character for credibility Ctesias has borne, not only with modern writers, but with those of his own country, from what may be considered almost his own age to much later periods of Greek literature. Aristotle declares he is unworthy of belief, Plutarch holds him as a mere fabler, and even the author of the Life of Apollonius treats him as "a liar of the first magnitude."

Ctesias, however, declares that he wrote nothing but what he had seen or heard; and, notwithstanding the unfavourable sentiments to which I have adverted, and in despite of Lucian's emendation of the declaration of Ctesias, making him assert that he had written what he had "neither seen nor "heard," I confess I am disposed to believe his averment. What he saw amounts to very little, and to nothing extraordinary. What he heard is much more copious, and more wonderful; but it was very possibly communicated to him in perfect good faith by persons who had visited the countries he speaks of, but who were persons of more credulity than observation. That Ctesias has repeated their marvellous narrations, may argue ignorance and credulity in him as well as in his informants, but it does not necessarily imply deliberate falsehood or mendacious invention.

Although, however, we may acquit Ctesias, and even those who imparted to him the strange stories he has repeated, of any settled purpose to deceive, yet it seems unaccountable how such impossibilities as he has recorded could have originated, how they could have been put forth in a country so near as Persia to the India to which they were ascribed, and with which there must have been frequent intercourse, both commercial and political; and how a man of the talents of Ctesias, a scholar and a physician, and imbued with a spirit of intelligent curiosity, could have been a party to their dissemination. To determine these points satisfactorily, it would be necessary to consider the state of society at the period at which Ctesias wrote, and the progress which the Persians and Greeks had made in physical science; but this would lead us away from the object of the present inquiry, which is restricted to the specific statements that Ctesias has transmitted; and it is sufficient for our purpose to ascertain whether they have been advanced wholly without foundation, or whether we may not discover in them some glimpses of the truth. That this has not already been accomplished in a satisfactory manner, need not excite any surprise; for the inquiry demands a familiar acquaintance with the varied subjects of the narrative, and with the country to which they belong; an acquaintance not within the reach of the earlier illustrators of our author, and even now but partially within our own. The India of Ctesias is but to a limited extent the India of our day. Except in a few instances, the Indus is to the southward the boundary of his information, and the regions to which he refers are those to the northward, along the upper course of the Indus, amongst the Hindu Koh and Karakoram mountains, Balkh, Bokhara, and the banks of the Oxus to the west; and to the east, Tibet, Badakhshan, Kashgar, and central Tartary to the confines of China. These countries have been hitherto but little accessible to Europeans, and we are too imperfectly acquainted with them to be able to avail ourselves fully of the means which they may possibly afford, successfully to elucidate the Indica of Ctesias.

Incomplete, however, as our materials may yet be, they are more ample than they have hitherto been. I have therefore thought it might be worth while to apply them to the illustration of the text of Ctesias, and that this Society might take some interest in the result, not only as calculated to throw some light upon the work of a classical authority, but as affording them an opportunity of contemplating some circumstances relating to India which may not heretofore have fallen under their notice.

The accounts of Ctesias, as they exist in the fragments preserved by Photius, or in the extracts from classical writers, collected by the industry and learning of modern editors and annotators, are put together in a most desultory manner, without any regard to subject or connexion. As it would be painful to follow such an unsystematic series, I have attempted to classify the different sections, and purpose to comment upon them under two principal heads: the first embracing all that Ctesias has stated regarding the country itself, and the people by whom it was inhabited; and the second comprehending his descriptions of the products of nature in the animal, vegetable, and mineral kingdoms.

INDIA AND ITS INHABITANTS.

AP. I.
bo XV.

"India," according to Ctesias, "was as extensive "as the rest of Asia; the people as numerous as all "the rest of mankind: and there was no nation be"yond them, or more to the east."

The correctness of these statements is to be estimated with reference to the prevailing notions of the extent and population of the globe in the days of the writer. At a much later period, when combating the exaggerations of Onesicritus, Nearchus, Deimachus, and Megasthenes, as well as those of Ctesias, Strabo concurred with Eratosthenes in extending the eastern limits of India, to the ocean [a], and assigning it a breadth of 16,000 stadia from north to south, and a length of 19,000 from east to west, whilst the whole breadth of the habitable world was not above 30,000, and the whole length 70,000 stadia, making India therefore more than half the width, and nearly one third of the length of the habitable globe. Agreeably to the notions of the Hindus, India would be still more extensive; for Aryavartta, the holy land, the country of the Hindus, is considered, in the Puranas, as bounded both upon the east and the west by the sea. The latter limit would carry India to the Mediterranean; and traces of this notion, although no longer entertained when Ctesias lived, may be discovered in classical as well as Indian authorities; for there were Indi in Colchis, in Mesopotamia, on the Tigris, and even in Thrace, according to ancient writers quoted by Bryant[b]. He therefore concludes that Chaldea was the parent country of the Hindus. "The arts and sciences," he says, "imported into India came from the Cuthites of "Chaldea:" and again, "One of the most considerable colonies that went from Babylonia was that "of the Indi, or Sindi, who settled between the "Indus and the Ganges: a large body of them "passed to the north, and thence extended themselves eastward quite to the ocean[c]." And this theory has recently received the countenance of an eminent oriental scholar, Col. Vans Kennedy, who traces the origin of the Sanscrit language to the country about Babylon[d]. It seems probable therefore, that ancient tradition, as well as imperfect geographical knowledge, had diffused amongst the Greeks indistinct notions of the vast extent of the Indian world, at a period prior to the more scientific cultivation of geography by the Alexandrian school, which would justify the statements of our author. Of the western limits of India, however, the times of Ctesias were able to form some tolerably correct idea; but to the east the boundary was still unknown; and even in the days of Pausanias, the country of the Seres, or China was supposed to be an island in the Erythræan sea[e], whilst Strabo[f] makes the prolongation of the chain of Taurus or the Himalaya, extending to the Eastern sea, the boundary of India. This sea, it is true, M. Gosselin[g] supposes to have been the bay of Bengal; but this is rather incompatible with his notion, that the Thinæ of Strabo, who occupied the extreme boundary, were the people of the Tenasserim coast, as that would then be the boundary of the ocean, instead of being bounded by it. Nor can it be supposed that the Indians themselves intended by their Eastern ocean the bay of Bengal; and we must therefore conclude, that it meant the China sea, or sea of Japan, and that China was included in the denomination, India, although its precise situation and extent may not have been accurately known.

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[a] Strabo, lib. i. xv.

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[b] Analysis of Ancient Mythology, vol. iv. p. 245, 252.

[c] Ibid. vol. v. p. 226.

[d] Researches into the Origin and Affinity of the principal Languages of Asia and Europe, p. 221.

Without, however, comprising China within the limits of India, there is no doubt that the latter country was more extensive even before the invasion of Alexander than it was subsequently, including Ariana, Arachosia, Margiana, Bactria, and even Sogdiana, and part of the Sacha regio, which were separated from it by the Macedonian conquests. In later times, the pressure of the Mongol and Turk tribes on the north, and the advance of the Mohammedans from the west, have still further contracted its boundaries, and detached Kandahar and Kabul from India, although they, as well as Bahlika, or Balkh, are enumerated amongst Indian countries in the Puranas; and the Sakas, the Σακαι, or Sacæ, are in the same works, and in the laws of Menu[h], described as Indians originally of the Kshetriya or military tribe, but who were deprived of caste by Sagara, a king of India, as a punishment for taking part in a successful insurrection against his father[i]. We may still discover in those countries names of places and of people, and traces of manners, evidently of Indian origin.

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[e] Bryant, v. 227. [f] Lib. xv.

[g] Géographie des Grecs. Mémoires de l'Académie, t. xlix.

At the same time that India was more extensive in ancient than in modern times, it seems to have been more populous, as far it was known. Besides the numerous armies brought into the field against Alexander, by princes ruling over very limited territories, it is said by Strabo that the Macedonians reduced 5000 considerable cities[k], and that Eucratides the Bactrian prince, reigned over 1000 in a region where scarcely a dozen places, that deserve the name of towns, are now to be met with, and where there are not above three or four cities, as Cabul, Peshawar, Lahore and Amritsar, that are of any consideration. The depopulation of Afghanistan and the Punjab is in a great degree owing to the state of anarchy in which they were involved upon the downfall of the Mogul monarchy, and in which the former at least is still plunged, whilst in the latter it has been only recently suspended by the talents of Ranjit Sinh, and is likely to recur upon his death. Similar causes have very much reduced the population of very extensive portions of India in other directions, and its surface is not without a due proportion of tracts of desert and wilderness, mountain and waste, where nature is inexorably opposed to the multiplication of human beings. Nevertheless, the average population of the whole country is still considerable. The latest accounts, and which, though necessarily imperfect, are best entitled to confidence, estimate the superficial area of India at above a million of square miles, with a population of 120 millions, or 120 to each square mile [l]; an average exceeding that of England and Wales a century ago. Where circumstances are favourable to increase, the population of India presents a ratio of the highest magnitude, and in the province of Burdwan is 600 to a square mile [m], although there is not a single town of any note in the district. Although therefore the amount assigned to the population of India by our author may not have been founded on very good authority, it was not very extravagant with reference to the population of those countries which formed the ancient world.

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[h] Institutes of Menu, x. 43, 44.

[i] Vishnu Purána, b. 4. c. 3.

[k] Lib. xv.

The only river of importance with which the Persians were likely to have been well acquainted was the Indus, and it does not appear that their knowledge of it was very correct. Ctesias says it was
I. 40 stadia broad in its narrowest, and 200 in its widest part. In citing the same passage, however, Arrian reads 100 stadia for the greatest breadth [n].

It is only very recently that we have become acquainted accurately with the course and extent of the Indus, by the travels and surveys of Lieut. Burnes[o], and have acquired the right as well as the ability to question the measurements of our author. At Attok, where the river is most usually crossed now, as it was in the days of the Macedonians, the breadth is but about 260 yards, in the month of June, when it is fullest; in March, it is not above 120. The greater of these two is very inferior to the 40 stadia of Ctesias, even if we were to adopt the proportion assigned to the stadium of this period by M. Gosselin; for at the rate of 1100 stadia to a degree of latitude, we have about 16 stadia to a mile, and consequently 40 stadia are equivalent to about 2½ miles. If we adhere, however, to the Olympic stade, it is above four miles and a half. At the same time we may make some excuse for the Greek writer, when we find a credible English traveller, Mr. Forster[p], who actually crossed the Indus above Attok, asserting that it was from three quarters of a mile to a mile in breadth, where not interrupted by islands. Some way below Attok, Messrs. Moorcroft and Trebeck found the river divided into two branches by a considerable island, and the nearest branch, although not above 100 yards or so in breadth, ran in a sandy bed that was 600 yards broad: when the whole bed is filled, therefore, the expanse of the river must be here at least half a mile. Even at Tatta, however, only 100 miles from the sea, the river is not above 700 yards across, and the greatest width which it ever attains in a single channel is not above 1000. There is one exception in the Koree, the most eastern limit of the Delta of the Indus, which below Luckput, suddenly opens like a funnel to a breadth of seven miles, and increases until one bank is no longer discernible from the opposite one. It is not likely, however, that this branch was known to Ctesias, and his measurement cannot apply to the whole of the Delta, for that, according to Arrian, occupied 1800 stadia.

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[l] Hamilton, Description of Hindostan; Journal of the Asiatic Society, Calcutta; vol. ii. 488.

[m] Asiatic Researches; xii. 551.

[n] Arrian, lib. v. de Exped. Alex.

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[o] Travels into Bokhara, &c.; Voyage up the Indus, and Memoir.

[p] Journey over land from Bengal to England in 1782, &c.

V. The Indian sea, it is said, is not less extensive than the Grecian sea. The Erythræan sea in the extent attributed to it subsequently to the days of Ctesias, appears to have comprised, along with the Red sea, that part of the Indian ocean between the straits of Babelmandel and the Malabar coast. The Indian sea of Ctesias may have been only the part between the Persian gulf and the coast of Malabar, and in that case might admit of some comparison with the Ægean. The water, he adds, is so warm for the depth of four fingers, that the fish cannot come to the surface. From experiments made by Messrs. G. and J. Prinsep, it appears that the mean temperature of the sea in the bay of Bengal exceeds that of the Atlantic, being in August, when it is probably at its highest, 81° 75′[q]; a temperature not sufficiently high to boil fish.

I. Of the climate of India it is said that "there is
" no rain, and that the lands are watered by the in-
VIII. " undations of the rivers; that there is neither thun-
" der nor lightning, but frequent gales and hurri-
" canes. The heat is so great, that in summer many
" are suffocated by it, and the sun seems to be twice
" as large as in other parts of the world. The rising

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[q] As. Res. xv. App. xvii.

"sun diffuses coolness, and for thirty-five days in the "year, at a certain place which is sacred to the sun "and moon, situated in an almost inaccessible tract, "at a distance of fifteen days' journey from the moun-"tains that yield the sardonyx, the sun restrains his "rays on purpose that pilgrims coming to an annual "festival held there may not be burnt alive."

In these accounts there is more of error than of truth, but they are not wholly without foundation. The countries along the Indus, although not absolutely without storms and rain, are especially subject to strong and dry gusts of wind, blowing up the sandy soil into hills, and the fertility of many parts of India depends much more upon irrigation and inundations than upon rain. A tract of between four and five hundred miles in length, and about two hundred in breadth, on the east of the valley of the Indus, is known to the Hindus as the *maru desa*[r], the dry country or desert; being for the most part an arid waste, thinly sprinkled with spots of cultivation, the miserable inhabitants of which, according to Col. Tod[s], calculate upon a partial famine every third year, and suffer occasionally under such an infliction three or four years consecutively. Similar sandy and desert tracts abound more to the north and north-west, dividing Bokhara from the Oxus and from the Caspian, and compared with which the Indian deserts of Jeselmer and Parkur sink into insignificance[t]. Even in more favoured tracts, or in the cultivated portions of the Punjab, Balkh, and Bokhara[u], productiveness depends more upon the supply of water from rivers and canals than from rain. Although it rains in the spring in Bokhara, yet the climate is arid, and the country would be uninhabitable, were it not for its rivers. In Shehr Sabz, where the canals cease, a sterile desert succeeds, and in Balkh the canals intersect the whole country.

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[r] Tod's Annals and Antiquities of Rajasthan, vol. ii. 289.

[s] Page 327.

[t] Burnes, ii. 11.

[u] Burnes, ii. 158.

With regard to the size of the sun, it is only said that it seems to be double its ordinary size; and it would have required good eyes to have accurately measured its dimensions in an Indian sky. It may however be observed, that in the regions of the East the minor planets shine at the dry season with great brilliance, in consequence of the clearness of the atmosphere, and may be thought to look larger and nearer to the earth than in more western climes. It is also true, that in those parts of India where the hot winds blow, the coolest hours of the twenty-four are those immediately preceding and following sunrise. It is much cooler then, for a brief period, than at midnight. The annual intermission of the sun's heat rather exceeds the period which Ctesias has assigned to it: and whatever may be thought of the cause to which it is ascribed, there are various places in India, sacred to both the sun and the moon, which were in former times objects of pilgrimage, although not so at present. Several such places are mentioned in the lists of Tirthas, or holy shrines, some of which are evidently situated in the north-west of India, although the situation is not distinctly specified. Amongst others in the Mahábhárata[y], we have the "abode of Aditya of accu-" mulated radiance, renowned in the three worlds,

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[y] Mahábhárata, Vana Parvan, vol. i. p. 529.

"where he who bathes and worships the sun secures for himself and his family admission to the "solar sphere;" and adjacent to it, "the holy shrine "of Soma (the moon), bathing and worshipping at "which elevates most assuredly a man to the moon." Although, however, there is no longer any shrine dedicated to the sun, nor any worship addressed particularly to any of the planets, yet it is admitted that all Sundays are sacred to the sun, as the name in Sanscrit, Aditya vára (आदित्यवार), as well as in other languages, imports; and that the Sundays in the month of Mágha (January) are so in an especial manner. The sun's entrance into the sign Capricornus, the Makara Sankranti, the winter solstice, is also still a festival; and people go to different holy places, as the mouth of the Ganges, to bathe and worship. It is clear, therefore, that in the passage under remark allusion is made to a season, and to a form of worship, which no doubt was popular in ancient times, and which is still, to a certain extent, observed by the Hindus.

Some of the most marvellous narrations of Ctesias relate to races of people by whom he says India is inhabited; yet even in them there appears to be a slight tincture of truth, or at least some foundation for the strange details that are given.

"The people," he says, "are black by nature, not IX.
"by the action of the sun. A few amongst them are "very fair (λευκοτάτοι):" and he mentions having seen two women and five men of such a complexion. There are Albinos in every part of India, the whole of whose skin is, as Dr. Ainslie[z] describes it, like that of a dead European who has not been much

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[z] Materia Medica of Hindostan. Madras, p. 300.

exposed to the sun. And Dubois[a] observes, that it is no uncommon thing to meet with a class of individuals much whiter than Europeans: they have light hair and weak eyes, but can see well in the dark. It might have been to some such objects that Ctesias refers: but if that was not the case, it is very possible that he might have met with Indians, whom, as contrasted with the swarthy complexion of the Persians, and of the Greeks themselves, he might have considered fair. Many of the people of the west and north of India, and of Turkestan, are not darker than the nations of the south of Europe, with a warmth of tint and a ruddiness of complexion that is not always found amongst the latter.

XIV. Of the manners and customs of the Indians, their justice, loyalty, and contempt of death, we have only the titles of the chapters. Devotedness to their employers and contempt of death are still their characteristics; and some remarkable instances of both have very lately occurred. The kings of the Indians, according to him, are not allowed to be intoxicated[b]. And drinking is one of the vices which Manu enjoins a king most carefully to shun: "With "extreme care let him shun eighteen vices; ten "proceeding from love of pleasure, eight springing "from wrath, and all ending in misery. Drinking, "dicing, women, and hunting, let a king consider "as the four most pernicious of those vices which "love of pleasure occasions[c]."

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[a] Description of the People of India, by the Abbé Dubois, p. 199.

[b] Apud Athenæum, lib. x.

[c] Manu, vii. v. 45—50. Sir Wm. Jones's translation.

"None of the Indians," he says, "ever suffer head- XV.
"ache or tooth-ache, or maladies of the eyes, or have "pimples about the mouth. They live to the age of "120 or 130, and some even to 200."

The simple diet of the people of India preserves them very generally from affections connected with disorders of the stomach; and they are remarkable for good teeth. Diseases of the eyes, however, are far from uncommon; and the duration of life is greatly exaggerated. Longevity, however, in the natives of the north-western provinces, is not rare; and the standard authorities of the Hindus regard a century as the natural boundary of human life; after which, voluntary death is not only excusable, but becoming: as it is said of king Sudraka, "Having attained the age of one hundred years and ten "days, he entered the fire[d]." The prayer to be addressed by its parent to a newly-born infant also says, "Thou art born of my body, my child, to live "for a hundred years[e]."

We next come to races of a different description, but who, amidst the cloud of fable which invests them, are very probably of Indian origin, either through the medium of fact or fiction.

"In central India," it is asserted, "a black people XI
"of pigmies reside, speaking the same language as "the other Indians, but of dwarfish stature, some of "them being not above a cubit and a half, and the "tallest not exceeding two cubits in stature. The "hair of their heads descends below their knees, and "their beards are longer than those of other men, "so that they use no garments, as they can wrap

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[d] Hindu Theatre, i. 15.

[e] Bagbhatta Uttara Tantra.

" themselves up entirely in their hair. They are " flat-nosed and ill-favoured, and are distinguished " by the disproportionate size of the sexual organ, " which descends to the ancles. The king of the " Indians has 3000 of these pigmies in his service, " as they are expert archers. They are an upright " race in their conduct, and observe the same laws " as the other Indians."

The belief that a people of Lilliputians existed, appears to have been very general amongst the ancients, and was very widely disseminated before the times of Ctesias. Various situations were assigned to them, and they were supposed to inhabit Thrace, Egypt, and Ethiopia[f], as well as India. Another circumstance, as old as Homer, is not mentioned by Ctesias, their being that " small infantry warred on " by cranes," or their combats with large partridges, which Megasthenes describes, or their riding upon partridges, or upon diminutive sheep and goats to the encounter, that others speak of. In fact, therefore, Ctesias deals less in the marvellous here than other authors. That the origin of this belief is to be traced partly to the real occurrence of dwarfs, and partly to the mistaking of monkeys for men, is exceedingly probable. Domitian evidently conceived the former notion when he set dwarfs to fight, and turned in cranes upon them[g]; and the soldiers of Alexander, it is recorded, actually charged a troop of monkeys, taking them for pigmy foes[h].

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[f] Iliad, b. 6. Aristot. Histor. Anim. viii. Juvenal, xiii. 167. Strabo, xv. Gerania in Thrace, an ancient city of the pigmies, Pliny, iv. 18: also viii. 2. Athen. ix.

[g] Statius, Sylv. i. 6. 57.

[h] Strabo, xv.

Dr. Tyson[i] identifies the pigmy and the Chimpansè, or large African monkey, but the majority of writers lean to the opinion that the prototype of the pigmy is to be sought for in the human race. Buffon thinks it probable that the fable of the pigmies contained some truth. Aristotle believed that what was related of them was fact, not fiction; and the Abbé Banier[k] concludes that the poets by whom the story was first narrated gave it an air of improbability by their exaggerations. "En un mot," he observes, "ils ont fait les géants trop grands et les pygmées "trop petits." Accordingly, the pigmy people are considered by Olaus Magnus to be the Samoyedes and Laplanders. Some German writers have placed them in the mines of Lusatia and Thuringia, in one of which Thurneisser has asserted[l] that the dead body of a man, two feet three inches long, was actually found. The object of the Abbé Banier's dissertation is to claim for the Pechini of Ptolemy, a diminutive people of Ethiopia, the honour of being the pigmies of Homer. Buffon[m] has given an account of a people of white dwarfs in the centre of Madagascar called Quimos, or Kimos, of whom one individual, a woman of mature years, who had been seen by the narrator, measured but three feet and seven or eight inches; and he also mentions a notice of a people in America, in Tucuman, bordering upon the Patagonians, some of whom had been brought by the Spaniards to Europe, whose height was under three feet. To these may be added the Papuas, or negroes of the Eastern islands, amongst whom a stature of four feet nine inches is that of a full grown male [n]. Whatever may be the value of these testimonies in favour of the ancient notion, as it regarded pigmy races in other parts of the world, it is foreign to our purpose to consider. It only concerns us to inquire what may be met with in India likely to elucidate the history of the pigmies in that country.

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[i] Essay on Pigmies and Satyrs.

[k] Mémoires de l'Académie des Inscriptions, v. 101.

[l] Mémoires, &c. v. 105.

[m] Histoire Naturelle, v. xi. 288

Dwarfs, some of whom are not much above three feet high, are not at all rare in India, and there, as well as in Europe in the middle ages, they formed part of a great man's retinue, at least in former times. Thus in the Ratnavali, when a monkey having broke loose alarms the household, "the dwarf," it is said, "creeps into the jacket of the chamberlain [o]:" and in the Viddha Sálábhanjiká [p], where a domestic picture of a prince, his queen, and attendants is described, the dwarf and the monkey are amongst the portraits. A dwarf, indeed, plays an important part in Hindu mythology, being the character assumed by Vishnu in one of his avatáras, or descents upon earth; and in the same mythology we have a still more diminutive race, the Balakhilyas, or Balikhilyas, pigmy saints and sages not bigger than the thumb, the sons of one of the patriarchs, Kratu, to the number of 60,000, attendants upon the chariot of the sun, and drinking his beams, or seated at the foot, or hanging on the branches of the Kalpa druma, or tree of heaven, immersed in contemplative devotion, or engaged in sacrificial rites;

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[n] Crawfurd. Indian Archipelago, vol. i.

[o] Hindu Theatre, 2.

[p] Ibid. 2.

and "weak masters though they be," endowed with super-human faculties: they are often mentioned in the Puránas[q], and in the Mahábhárat a story is related of Indra, the king of the gods, having given them offence, in consequence of which he was nearly deposed from his high estate. The occasion is thus "related: Indra and the pigmies having been directed "by the patriarch Kasyapa to bring fuel for a sacrificial fire, Indra performed his task readily, but he "was amused at beholding his companions, a number of grave, half-starved looking creatures of the "height of the thumb, labouring under the burden "of a single Palása stick, and struggling through "a pool of water collected in the impression made "by the foot of a cow. He went to their aid, and "lifted them and their load out of the puddle, but "in so doing he laughed, and they were grievously "displeased[r]."

It is not impossible, however, that some of the notions relating to the Indian pigmies might have been gathered from Indian architecture, in which it is very common to see dwarfish figures of grotesque visage and uncouth forms, and with gross exaggerations of some of their members sculptured either in relief, or as statues, representing attendants upon the divinities, dwarapalas, or door-keepers, and religious ascetics. Some of the latter also, even in their living condition, the gymnosophists of ancient, and the Sanyásis of more modern times, black, ugly, and emaciated, and having no other vesture than overgrown beards and hair, might have furnished some of the features of our author's description.

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[q] Vayu Pur. c. 29. Kurma P. Vishnu P. b. iii. &c.

[r] Mahabharata. Adi Parvan. p. 53.

There is yet another and very probable source from which some of the circumstances may have originated, and the pigmies of Ctesias *in the middle of India* may have been suggested by the Bhils, Gonds, Koles, and other tribes, who inhabit the almost inaccessible hills and impenetrable forests of the different branches of the Vindhya mountains, extending across central India from Bengal and the Ganges to the gulf of Cambay and the coast of Malabar. These people average much less than the middle stature; they are of a very black complexion and uncouth appearance; they commonly go naked, or very nearly so, and have sometimes long shaggy beards and hair: they are all expert archers. The progenitor of these races is fabled in Hindu legend to have sprung from the left thigh of Vena, an impious raja, who had been put to death by certain holy sages. They then set to work to knead and rub his limbs, in order to provide a successor, and the first product, or his impersonated iniquity, was this ancestor of the barbarian denizens of the mountain and forest. After describing the proceedings of the rishis, or sages, the Bhágavata Purána continues: "Then arose a person of dwarfish stature and di" minutive limbs, of a complexion as black as a crow, "with short arms and projecting jaw, stumpy feet, "a flat nose, red eyes, and copper-coloured hair. "He was called Nisháda, and was the ancestor of "the inhabitants of the forest and the mountain." The Padma Purána[s] adds, "that he was of hideous "aspect and limited understanding, and was armed "with a bow and arrows, and that he was the

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[s] B. iv. 14.

"progenitor of the Chándálas (or outcastes) and "Nishádas (or foresters), races addicted to vicious "practices, and devoid of all law, civil or religious." The Mahábhárata has similar accounts of the population of Gondwana. Hamilton[t] remarks, "the country of the native Gonds remains for the most part "a primæval wilderness, its human inhabitants being "scarcely superior to the brutes, with which they "live intermixed." Malcolm[u] terms the Bhils "a "diminutive and wretched-looking race:" they are professed robbers and thieves, armed with bows and arrows. In 1831 some predatory incursions of the Lurka koles, on the western frontier of Bengal, rendered it necessary to send troops into their thickets, and they were found to be a short, black, ugly people, scarcely human in their appearance, but skilful archers. The hill-people of Rajamahal whom I have seen are much superior to the other mountain tribes, but few of them attain the height of five feet, and they are very black, with thick lips and flat noses. These races were much more widely spread over India in former times, especially in the central provinces, of which they appear to have been the earliest known, if not the aboriginal inhabitants. Their employment by the king of the Indians, of which Ctesias speaks, is a characteristic which they have long retained. They are introduced constantly into plays and poems as the guards and personal attendants of princes. In the Vikrama and Urvasi[v], Pururavas enters attended by a védhaka, a hunter or forester, the vedda of the peninsula, and weddah of Ceylon; and in the Retnavali [x] the guards of the inner courts of the palace are kirátas, or woodsmen. In Col. Tod's Rajputana, the Meenas, a tribe of foresters, are described as retained in the service of the rajas of Delhi and Udayapur; and it appears that men of this tribe, attached to the palace of Jaypur, have been the chief agents in a late attack upon the British Resident, and the murder of a young civilian by whom he was accompanied. A similar policy has been adopted even by the British government, and the hill-men of Bhagalpur and the Bhils of Kandesh have been embodied as a local militia; the dwarf, if not the pigmy bow-men of the king of the Indians.

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[t] Description of Hindostan, 2. 6.

[u] Account of Central India.

[v] Hindu Theatre, i. 260.

Of another race of Indians particularized by Ctesias we might reasonably despair of discovering any existing prototype; and yet, omitting what is hopelessly fabulous, it may not be impossible to conjecture an origin even for the Kalystrii, or Kunokephali,
XX. kephali, the dog-headed people [y]. "These are said to "inhabit the mountains that extend to the Indus to "the number of 120,000: they have the heads of "dogs, with large teeth and sharp claws, and their "only language is a sort of bark. They are said to "be very honest, and to maintain a commercial intercourse with other Indians who understand their "meaning partly by their bark, and partly by signs. "They are clothed in the skins of wild animals, and
XXII. "feed upon their raw flesh. It is stated in another "passage, that they bake it in the sun. They follow "no occupation, but live by the chase, and rarely fail

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[x] Hindu Theatre, 2.

[y] Ælian. iv. 46. Plin. vii. 2. Diod. i. 3. Philos. 217.

" of their prey, as they are sure archers, and very
" swift of foot. They rear sheep, goats, and asses,
" and they drink ewe's milk and curds. They feed
" also on the fruit of the Siptakora, which they dry
" like raisins, and send a quantity of it annually to
" the king of the Indians packed in baskets which
" they fabricate, along with a quantity of the flowers
" yielding a purple dye, of an insect whence a red
" dye is extracted, and of amber, and these are floated
" on rafts down the river. They barter the same
" articles, and swords, bows, and arrows of their own
" manufacture for bread, meal, and cotton garments.
" Their situation amongst lofty mountains makes
" them invincible: the king supplies them every fifth
" year with 300 bows and as many darts, 120 shields,
" and 50,000 swords. They have no houses, but re-
" side in caves. The men never wash any part of
" their persons, and the women only once a month,
" but both sexes smear themselves with oil thrice
" a month, wiping it off with skins. The wealthy
" wear cotton garments, but the greater number, both
" men and women, are dressed in skins, the thinnest
" procurable from which the hair has been removed.
" He is the most opulent who has most sheep. Both
" men and women have tails like dogs, but longer
" and more hairy. It is amongst these people espe-
" cially that extreme longevity is said to prevail.

" Beyond these, and nearer to the sources of the XXIV.
" river, are others who are very black, and neither
" eat corn nor drink water, but rear flocks of sheep,
" goats, and oxen, and live entirely upon their milk.
" It is said that they void no ordure, being born with-
" out any aperture for that purpose, and that the ex-
" crementitious portion of their food passes off either

" by the urine, or is ejected from the stomach by the " emetic properties of a sweet root which they mix " with their milk at noon and in the evening. At " the latter season it operates as an emetic, at the " former, it prevents the milk from curdling in the " stomach.

xxvi. " In the mountains whence comes the Indian reed, " there are not fewer than 30,000 people whose women " bear but once in their lives; the children are born " with teeth, and the hair of the head and eyebrows " is grey at birth, but becomes black from thirty to " sixty. These people have eight fingers on each " hand, and as many toes on each foot. They are " very warlike, and the king has in his service 5000 " of them as archers, besides spearmen. Their ears " are said to be so large as to cover their shoulders " and arms as low as the elbows.

xv. " Of the Seres, and the remoter Indians, it is said " that they are of gigantic stature, some of them " being thirteen cubits high. Men with tails, like " satyrs, are also reported to exist in the interior of " some island in the ocean.

" Another race of Indians, the Cynomolgi, are said " by Ælian [y], on the authority of Ctesias, to rear a " large breed of dogs, in order to protect themselves " against the periodical incursions of wild cattle, who " during the latter part of the summer are driven " mad by the stings of bees and wasps. The dogs " alone are able to keep them off, killing many of " them, whose flesh is partly eaten by their masters. " At other seasons these people hunt other wild animals by the aid of their dogs, and also drink the

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[y] Ælian, xvi. 31.

" milk of the latter." According to Julius Pollux, the Cynomolgi were the dogs, not their masters. Passages in Pliny[z], apparently derived from Ctesias, notice other monstrous people, or the Monosceli, who, although having but one leg, are wonderful leapers. The Skiopodæ, whose feet are so broad, that when the person lies on the ground, and elevates his leg, his foot answers the purpose of a parasol. And a third race, who have no necks, and carry their eyes in their shoulders.

In these accounts there are of course many absurdities which it would be in vain to attempt to explain, and all that can be said in behalf of Ctesias is, that he is kept in countenance by his countrymen, of whom Strabo[a] remarks, that in general those who have written concerning India are liars, especially Deimachus, Megasthenes, Onesicritus, Nearchus, and others of the same stamp. Even Herodotus is not free from incredible fictions, but Megasthenes and Deimachus seem especially to have been authorities for such marvels as men wrapped up in their ears, destitute of mouths and noses, one-eyed, long-legged, and having the fingers bent backwards, satyrs with square heads, serpents swallowing oxen, horns and all, pigmies, and gold-making ants. Amidst these extravagances, however, there are some vestiges of fact, and of the incredible parts it may be suspected that many of them have at least a local propriety and are of Indian origin, the inventions of Hindu superstition having been mistaken for truths by the credulous ignorance of the Grecian ambassadors.

[z] vii. 2.

[a] Lib. ii. and xv.

All modern travellers in the countries within or to the north of the Himalayan mountains, to which the older stories seem chiefly to relate, agree in representing the people as a remarkably indolent and dirty race. The men leave to the women the labours of the field, and remain idling at home; and both sexes[b] exhibit a marked aversion to the use of water in ablutions of any kind, and substituting unctions of grease or oil. They are clothed for the most part in skins, with the hair inwards; whence perhaps the notion that it was removed. In warm weather, those who can afford it dress in cotton chintzes, manufactured in the plains, and procured by barter. They have little agriculture, and subsist, in a great measure, upon the produce of their flocks and herds, especially in the form of milk and butter. The use of tea very generally prevails; and this is perhaps the herb, rather than the root, which qualifies the milk, and prevents its coagulation, as well as occasionally relieves the stomach of its contents. The Bhotias and Tibetans exchange for cotton manufactures, and for rice and wheat, their wool, their cattle, and their dried fruits; and the intercourse with the people of the plains is carried on to a great extent by signs, as they speak languages unknown to their more southern neighbours. In the present day, they generally reside in log houses, or huts; but the mountains are in many places pierced with caves, which have apparently been the dwellings of a troglodyte population. At Bamian, for example, Lieut. Burnes describes the hill as quite honeycombed with excavations; and states that the caves altogether form an immense city.

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[b] See the Travels, recently published, of Captains Munday, Archer, and Skinner.

Even their having the appendage of a tail appears to admit of explanation, if the same costume which prevails in the present day existed in the days of Ctesias; and fashion in these regions is not entitled to the character of changeableness. Most of the hill tribes, both men and women, wear their hair plaited behind into one long tress; and this being elongated by worsted twist and tassels descends half way down the thigh, or even lower, suggesting not unnaturally the notion of a dog's tail. A remarkable illustration of the insular satyrs of our author is of modern occurrence. It is not a century since that a lieutenant of a Swedish vessel asserted of the people of the Nicobar islands, that they had tails like cats, which they moved in the same manner. Linnæus vouched for the narrator's honesty, and Lord Monboddo [c] exulted in his evidence as decisive of the question. The mystery is thus solved by Mr. Fontana, who, describing the people of the Nicobars, observes of their dress: "A long narrow cloth, made of the bark of "a tree, round their waist, *with one extremity hanging down behind*, is all their dress [d]." Lieutenant Keeping saw the people only from the ship; and the blunder was pardonable in a person impressed probably by the previous assertions of Careri and Struys, Marco Polo and Ptolemy [e], with a belief that men with tails had a real existence.

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[c] Origin of Language, part I. b. ii. c. 3; and Ancient Metaphysics, iii. 250.

[d] Asiatic Researches, iii. 151.

[e] Buffon, Hist. Natur. de l'Homme, vol. v. p. 45.

The dog's head appears to have not improbably originated in a verbal blunder. Kalystrii is given as the native name, meaning in the Indian language, according to Ctesias, κυνοκεφαλοι; and the question is, how far he or his informant have accurately written or explained the word. Some distinguished scholars and Orientalists, as Reland in his Miscellaneous Dissertations[f], and Tychsen in the Appendix to the second volume of Heeren's Historical Researches[g], have expressed an opinion, that all the foreign words which occur in Ctesias are not Indian, but Persian. That one or two are Persian may be admitted; but there is no reason to question the Indian origin of several of them: and the attempts of the writers in question to assign a Persian etymology to the greater number have been exceedingly unfortunate. The word *kalystrii* is an instance. Reland would derive it from *kalleh shikari*, کلّه شکاري, which, he says, means "caput canium unde contracte Kaliskaroi scripsit Ctesias et "per incuriam librariorum Kalustrioi." But even if his gratuitous correction of the reading were admissible, his etymology is not; for *kalleh* means rather the crown of the head, than the head: and *shikari* means, *hunter*, *hunting*, any thing belonging to the chase, not a dog in particular. Tychsen proposes either *kelek-sir*, كلك سر, *wolf-headed*; or *kalus*, كالوس, *foolish*, *stupid*; in the superlative form kalusterin, كالوسترين, *very foolish*; converting *dog-head* into *block-head*. He is avowedly dissatisfied with either of these conjectures, and they are by no means satisfactory. The ingenuity of Col. Vans Kennedy[h] has supplied a much more probable origin in the Sanscrit *kála-vastri*, easily convertible into *kalustri*, as *v* and *u* are interchangeable letters. The sense of the compound, it is true, is not "having the head of a dog," but "having black "raiment;" and this would be fatal to the identification, if the interpretation of Ctesias were to be relied upon. There is, however, in favour of the affinity an argument of more weight, that Col. Vans Kennedy has not adverted to it, and by which therefore he was not previously biassed in proposing the Sanscrit compound. This is the existence of a people, inhabiting a mountainous district in the direction to which Ctesias refers, who have been known certainly for five centuries by the term in question. These are the people denominated by Mohammedan writers, and by the people surrounding them, the Siah-posh Kafirs, *the black-vestured infidels*. At the end of the 14th century[i] they provoked the wrath of Tamerlane, on his way to invade Hindustan, and were thence brought to the knowledge of the Persian historians. They are described as a brave though barbarous people, speaking a language peculiar to themselves, and occupying narrow valleys, amidst lofty and almost inaccessible mountains. Although unable to contend with the overwhelming power of the Tartar monarch, they were not reduced without difficulty and loss. At a later period the Siah-posh were said by Baber and Abulfazl to be the descendants of the Macedonians; but the inquiries of Mr. Elphinstone[k], when on his embassy to Cabul, induced him to disbelieve the tradition. Lieut. Burnes also denies this descent of the Kafirs[l], although he thinks the pretensions of the chiefs of Badakhshan and the valley of the Oxus, which were first noticed by Marco Polo, better founded. Of the black-vested Kafirs he remarks, that they appear to be a most barbarous people, eaters of bears and monkeys, fighting with arrows, and scalping their enemies, circumstances quite in harmony with the character given by Ctesias of the Kalystrii. They are fairer than most Asiatics; and a Kafir boy of ten years of age, whom Lieut. Burnes met with, differed in complexion, hair, and features, from other Asiatics, and had eyes of a bluish colour, affording some authority for the white-complexioned children mentioned by Ctesias, those of a people who Pliny asserts were called by him *Pandoræ*, a genuine Sanscrit word, *Pandura* meaning *pale* or *fair*. Lieut. Burnes supposes the Siah-posh to have been the aborigines of the plains, who fled to the mountains from the advance of the Mohammedans. From information obtained by Messrs. Moorcroft and Trebeck, when in Little Tibet, it appears that the Siah-posh Kafirs are nothing more than a tribe of the people called by the Hindu geographers, both in past times and in the present day, Dáradas or Durds, दारद and who have borne that appellation from time immemorial, being the Daradræ of Ptolemy, situated at the sources of the Indus; and the Dardai of Megasthenes, as quoted by Strabo[m], who inhabited the country of the gold-making ants. Now the sense of Dárada is *tearer*, *render*, from Dri, *to tear to pieces;* and this name, which is no doubt as old as Ctesias, may have contributed to form the canine teeth and talons of the people so called: whilst their other appellation, Kálavastri, indicating the usage which they still observe, and whence they are called by their neighbours Siah-posh, *black-vested*, that of *wearing black goat-skins*, furnished the denomination, Kalystrii, although the purport of it was inaccurately explained.

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[f] Dissert. de veteri lingua Indica, i. 209.

[g] Historical Researches by Heeren, ii. 376.

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[h] Calcutta Quarterly Magazine and Review, June 1827, p. 218.

[i] Rozet al Sefa.

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[k] Embassy to Cabul. Account of Kaferistan, 617.

[l] Travels to Bokhara, ii. 210.

[m] B. xv.

Of the other extraordinary tribes of Indians, whose existence rests either upon the authority of Ctesias or of subsequent writers, the origin may be with every appearance referred to India, being in one or two instances perhaps misrepresentations of facts, but in the greater number the assignment of a local habitation to the creations of Hindu fable. Thus, for the people with large ears, we find a class of demigods called Hastikarnas, *elephant-eared;* and large and long ears are features frequently ascribed to preternatural agents in Sanscrit fiction. For the Acephali we have the Kabandha, a headless but living trunk. One such personage is described in the Rámáyana [n] as seizing upon Rama, and his brother and wife, at the commencement of their exile: and another occurs in Ráhu, the personified descending node [o]: his head was struck off by Vishnu; but it was after he had quaffed the Amrita, or beverage of immortality; and head and body thence enjoyed a durable though divided existence. Horned satyrs appear in the Bhagavat, and Monoculi in the Kálíka Purana. As for a superfluity of limbs, and for monstrosity of form, there is an infinitude of such beings in the Hindu Pantheon, from the chiefs downwards to the meanest hobgoblin. Brahmá has four heads; Siva three eyes; Vishnu, in his Avatars, has the tail of a fish, the body of a tortoise, the head of a boar, the head and talons of a lion; Ganesa has the head of an elephant. Amongst the Dánavas, or children of Danu, the Titanic foes of the gods, we have one with the head of a tiger, one with the head of a goat, another has two heads, and a fourth has ears like sharp spikes[p]. Another of the wives of the patriarch Kasyapa, Khasá, is the mother of two sons, Yaksha and Rakshas; the former has two heads, four arms, four feet, enormous ears, and a proboscis like an elephant's; and the latter is a dwarf, with three heads, three feet, large ears, and disproportionate members: and these two were the progenitors of races equally monstrous. The demigods attendant upon Indra and Kuvera, the deities of heaven and of wealth, may well be termed Kinnaras and Kimpurushas, or literally *What-men, How strange a sort of beings!* for their synonymes are Aswamukha, Hayánana, and similar compounds, intimating that they have the heads of horses, from Aswa, or Haya, *a horse;* and Mukha, or ánana, *a face.* In the Bhagavat, the beings whom Indra sends to frighten Dhruva, and interrupt his austerities, have the heads of tigers, bears, and elephants, and the bodies of buffaloes, cats, and owls. And in the Kálíka Purana the train of Káma, the Hindu Cupid, instead of the loves and graces, consists of the fiercer passions personified as beings with the heads of jackals, cats, and monkeys. Some of them have one eye, some have three, some have immense ears, some have one arm, some have many arms, some are giants, and some pigmies. The beings, too, that are supposed to live upon earth, haunting charnel houses and cemeteries, or the places where the dead are burnt, Pisáchas and Bhútas, besides being anthropophagi, are described as of gigantic stature and monstrous form; and these, and the races previously alluded to, may, either through oral description or sculptured representation, have borne the character of existing realities to superstitious and ill-informed traders and travellers.

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[n] Rámáyana Aranya Káñda.

[o] Vishnu Purána.

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[p] Váyu Purána.

With regard also to realities, we may perhaps trace the Monosceli to Hindu asceticism. One of the favourite exercises of a person performing tapas, or penance, is standing on one foot. This, in the legendary accounts of the austerities of various persons, who seek to compel the gods to grant them a boon, is done for a thousand years, and, in imitation of their devotion, for such period as is practicable by mere mortal sages. Representations of ascetics in this posture were probably frequent. Figures of Krishna and of Siva dancing on one leg also occur; and one of the names of the latter is Ekapád, Monoscelus. Again, notwithstanding the authentic portraits given us by learned men[q], of the umbrella footed gentry, it seems very probable, that, as Buffon has imagined, they originated in the diseased enlargement of one of the legs, the Cochin leg, as it is popularly termed [r], but common in other parts of India besides Malabar; and which, though sometimes connected with elephantiasis, is not necessarily so. It is not unfrequently "a chronic complaint, interfering in no degree with the functions of life [s]." It is not very clear what may be meant by the men with reverted fingers (*οπισθυδακτυλοι*), but they may perhaps be intended for the Urddhabáhus, ascetics, who hold up one arm until they cannot bring it down again, and keep their palms closed until the finger nails make their way between the metacarpal bones and appear at the back of the hand. However this may be, we can scarcely doubt that the Macrosceli, the long-legged race, were derived from a real source, as the word is little more than a translation of the Sanscrit *Tála jangha*, (तालजंघा:) people having legs like palm trees; the metaphorical denomination of a Hindu tribe which makes a conspicuous figure in the ancient history of India.

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[q] They are engraved in Munster's Cosmographia, and Lycosthenes de Prodigiis, as pointed out to me by Professor Rigaud.

There is nothing else in Ctesias relative to the human species which is of any importance. "The Indians," he says, "make very good wine and cheese, "and he had tasted them." We need scarcely call his veracity in question here, whatever we may think of his taste. Cheese and wine are not manufactured in India Proper, but they are to the northward. The people of Bokhara use much cheese, according to Lieut. Burnes [t], and he also mentions the wines of Bokhara, which, he says, are unpalatable to European taste. That they were not so to Asiatic taste we have high authority; for the emperor

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[r] Hist. Natur. de l'Homme, v. 64.

[s] Trans. Medical and Physical Society of Calcutta, i. 14.

[t] Travels to Bokhara, ii. 183.

Baber, who was in the early part of his life a boon companion, thus speaks of it: "In all Maweralnahar, there is no wine superior in spirit and "strength to that of Bokhara. When I drank "wine at Samarkand, in the days when I had my "drinking bouts, I used the wine of Bokhara[u]." Ctesias is therefore fully borne out in this particular, at least, by unexceptionable testimony.

NATURAL HISTORY OF INDIA.

We next come to our author's natural history, and first, of his accounts of the animals of India.

VII.
Ælian IV. 21.
Animals.

In this division of our subject, the foremost place is due to the formidable creature he describes under the denomination of Martichora, "an animal said to "have the face of a man and the bulk of a lion; to "be of a red colour, having three rows of teeth in "each jaw, with human ears but larger, and with "grey eyes; equipped with a tail above a cubit long, "pointed with a sting like that of a scorpion, and "armed with upright and transverse spines, a wound "from any one of which is fatal. When assailed by "a distant foe, the Martichora turns its tail in that "direction, and darts its spines from it like arrows "from a bow. It can cast these spines to the dis"tance of a plethrum, and they kill any animal ex"cept an elephant. The spines are a foot long, and "of the thickness of a slender cord. The meaning of "the martichora is *man-eater* (ανθρωποφαγος), as it "devours many of those whom it has slain; it also "preys upon animals. It fights with its talons as "well as its spines; and when any of the latter have "been shot away, others grow in their place. India

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[u] Memoirs of Baber, 53.

"abounds with these animals, and they are often killed "with darts by men who hunt them on elephants."

Amidst the carnivora of the Indian forests, it would be somewhat difficult to discover the Martichora, and yet it would appear to be not altogether the product of our author's imagination. Indeed Ælian makes him assert that he had actually seen the animal, one having been sent from India as a present to the king of Persia. It may be, however, that he asserted he had seen only the representation of such an animal, which would accord with Heeren's theory of its origin[v], who describing the two sculptured figures on the east portal of the ruined buildings at Persepolis, delineated by Niebuhr, Morier, and Porter, and which have human faces, with the bodies of lions, observes, that the former characteristic convinces him that they were meant for the Martichoras of Ctesias, who in his narratives has here preserved legends current in Persia, derived from a mythological system of Oriental, Persian, or rather of Bactro-Indian origin. The elements of this mythological creation were, he adds[x], all real animals, the lion, the bull, the horse, the onager or wild ass, the rhinoceros, the ostrich, the eagle, and the scorpion, blended together in a variety of monstrous combinations, to which the fancy of the poet or the artist superadded a number of capricious ornaments. Although this may be true, yet the Persepolitan figures, being furnished with wings, and devoid of tails, can scarcely be considered as the prototypes of the Martichora, and we may still look to India for the original. Pausanias[y] thinks it is nothing more than the tiger; but although the tiger may have contributed to its formation, yet some of the appurtenances cannot be traced to that source. The tail and its spikes seem rather to have been suggested by some imperfect reports of lizards or alligators, or still more, of the porcupine, which is well known in India, whence, it has been supposed, it was brought into Europe. It is found both in the plains and in the mountains. A belief in its power of shooting its quills, and of their deadly property, is as prevalent in India as it has been in Europe, and one of its names is Swá-vit, *dog*, or *wild beast piercer*. Others of its names are Saláká and Salya, meaning *a javelin* or *dart*. The body of the Martichora may be borrowed from a real animal, or from the Sinha, the mythological lion, on which the goddess Durgá rides, and the human face may have been a Persian embellishment, if it did not originate in some grotesque specimen of Indian sculpture. It can scarcely be doubted that Martichora, which seems to be the preferable reading, although it occurs Mantichora in Aristotle and Pliny[z], is a Persian term. Etymologists are here agreed. Reland[a] observes, "Hodie quæ mirabilis est linguæ Persicæ constantia *ανθρωποφαγος* مرد خورا Mardi chora "appellatur a مرد *homo* et خوردن *edere*," and Tychsen[b] says Martichora is neither more nor less than the Persian Mard-khora, مردخورا, from مرد, *a man*, and خوردن *to eat*. Mardam-chora, مردم خورا also, as he observes, is a compound found in Persian writers to denote a fierce warrior. Thevenot, giving an account of some tribes in the south of India, who were accused of cannibalism, asserts that they were called Mardi khora by their neighbours. The latter member of the term is in frequent use in India, in the sense of *eater*, as in the word halal-khor حلال خور, the name of a low caste, so called because they eat every thing, every thing is lawful food to them. If we admit that the Persian of the time of Ctesias was the Zend, we still preserve the affinity; for according to Du Perron's Zend vocabulary[c], the words would be mereté-khore. The same may be traced to a Sanscrit source, मर्त्य or marttya, *a mortal, a man*, and khâda खाद् *who eats*, marttya khâda; and there is no lack of anthropophagi amongst the creations of Hindu fable. The Rákshasas, Pisáchas, and Bhutas, the giants, ogres, and goblins of the Hindus, are all cannibals, as in the Bhatti, Khara and Dúshana, two Rákshasas, observe, "Day and "night we devour men, nor are we therein to "blame." The Bhágavata, describing such a being, says, "He drank the blood from the entrails, and "crunched the bones like lotus stalks;" and Mádhava, in the drama of Malati and Madhava[d], seeking to propitiate the spirits of ill, brings them raw flesh as an offering:

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[v] Vol. i. 154.

[x] i. 152.

[y] In Bœoticis.

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[z] Aristot. Hist. Animal. ii. c. 1; Pliny, viii. 21.

[a] Dissertations, vol. i. 223.

[b] Heeren, ii. 377.

> "Demons of ill, and disembodied spirits
> "Who haunt this spot, I bring you flesh for sale—
> "The flesh of man."

Men also may be condemned for a period to similar propensities; and Kalmáshapáda[e], one of the early kings of India, became a cannibal at the command of his spiritual preceptor, whom he had displeased, for a period of twelve years, during which, among others, he devoured his teacher's sons. Some such stories may have contributed the human heads to the body and talons of the real lion or tiger; and the rest of the martichora have been compounded of the tail of the scorpion and the spines of the porcupine.

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[c] Zend avesta, vol. ii.

[d] Hindu Theatre, ii. 56.

[e] Mahábhárata, vol. i. p. 243.

III. There was a chapter, of which the title only remains, concerning elephants who break down walls, τειχοκαταλυτων ελεφαντων. Ælian[f] adds, that they are trained to do so at the command of their princes, by applying their breasts against them. Elephants have been constantly employed in Indian warfare to force open the gates of fortified buildings, and it appears to have been a practice to exercise them against artificial ramparts or mounds. Thus in the Megha Duta, a large cloud sailing along the sky is compared to an elephant butting at a mound:

> . . . . He beheld a cloud,
> In form some elephant, whose sportive rage
> Ramparts unequal to his might engage—

and *Vaprakrira*, literally, playing at or against a mound, is used to signify, the butting of an elephant. Some other particulars are cited from Ctesias by Ælian and Aristotle. The former states, that Ctesias relates having seen an elephant, at the command of his driver, overturn a palm-tree by a violent shock: a not improbable occurrence. The latter[g] denies the accuracy of the accounts which Ctesias has given of the procreation of these animals, and of the seminal excretion of the male becoming hardened like amber. As no other particulars are given, we cannot vindicate our author's correctness; but in the latter case it is possible that some misapprehension has occurred, and that the excretion alluded to was that which issues at certain seasons from the elephant's temples, and hardens like gum upon his cheeks; a physiological peculiarity frequently noticed by the Indian poets, as the excretion is supposed, by its fragrance and sweetness, to have a strong attraction for bees, who thence infest the animal's head, as thus described in the Ritu Sanhara:

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[f] xvii. 28.

[g] B. ii. c. 2.

> Roars the wild elephant, inflamed with love,
> And the deep sound reverberates from above;
> His ample front like some full lotus blows,
> Where sport the bees, and fragrant moisture flows[i].

The circumstance is noticed by Strabo[k], but escaped all subsequent writers until the days of Cuvier[l]: according to him, the exudation is any thing but fragrant.

Ctesias, as Schlegel observes[m], in his history of the elephant, is the first writer who has described the employment of elephants in war; for their employment in this capacity by the Greeks was one of the consequences of Alexander's invasion. As quoted by Ælian, however, our author has exaggerated the numbers attached to the service of the king on such occasions: he makes them 100,000, a much greater establishment than Indian princes were ever known to possess. The number brought into action in India by Porus amounted to 200; but the Macedonians were terrified by the report that the king of the Gangaridæ could bring 6000 into the field. The highest of their numbers, however, leaves Ctesias chargeable with extravagance; and all that can be said in excuse is, that the exaggeration was probably Indian. According to Hindu writers, an *Akshauhini*, or full army, is formed of 109,300 foot, 65,610 horse, 21,870 chariots, and as many elephants; and in the Mahábhárata, which records the struggle for the paramount sovereignty of India between the kindred families of Kuru and Pandu, in which all the rajas of India took part, the contending armies comprised many Akshauhinis, affording an original authority for 100.000, or even a greater number, of war-elephants being in array.

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[i] Megha Duta, or Cloud Messenger, p. 28. n. [k] xv. 32.

[l] Ménagerie, t. i. p. 121. [m] Indische Bibliothek. vol. i. p. 2.

Another section treats of small monkeys having tails four cubits long. There is no want of monkeys of various species in India, but the long-tailed monkey, or Hanuman (Entellus), is not one of the smallest. III.

A chapter was devoted to Indian dogs, of whom the only particular that has been preserved is, that they were so large that they could fight with lions. That the Indian dog was highly prized by the Persians before the time of Ctesias, we know from Herodotus, as the animal was an article of import, and large packs were kept by the Persian king and nobles[n]. As these were dogs for the chase, they could not have been of the species of the wild dog of India; for this is an animal which it has been found impossible to tame. The Indian dog of the Persians must have been rather the dog of Tartary, of whom various breeds were kept for the purpose of hunting, as late as the thirteenth century; the grand Khan, according to Marco Polo, taking the field with a pack of 5000[o]. According to the same traveller, there are dogs in the province of Centigui[p], of such strength and prowess, that two of them are more than a match for a lion. The Tibetan dog is an animal of great strength and fierceness, though capable of domestication. Of the wild dog of India we have only lately had any authentic description; an account of the *buansu*, or wild dog of Nepal, having been published by Mr. Hodgson[q], and another of the *kolsun*, or same animal in Malabar, by Col. Sykes[r]. That V.

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[n] Herod. i. 192. vii. 187.

[o] Ramusios' edition, p. 28.

[p] Ibid. p. 40.

[q] Asiatic Res. xviii. p. 2.

[r] T. R. As. Soc. vol. iii.

these dogs hunt other animals in packs of eight or ten, and overpower not only buffaloes and wild cattle, but even leopards and tigers, was first stated by the author of the Wild Sports of the East; it was confirmed by bishop Heber; and is placed beyond doubt by the observation and inquiries of the last named authorities.

XIII. "Indian sheep and goats, it is said, are larger "than asses, and bring forth four or six lambs at "most. They have tails so large, that it is neces-"sary to cut them off from the female, to admit the "access of the male." On the other hand it is stated, that "amongst the pigmies the cattle are of the like "diminutive stature, the sheep are no bigger than "lambs in other countries[r], and the oxen, asses, "horses, and mules, and other beasts of burden, "are not larger than ordinary rams. There are no "swine, either wild or domestic."

Now it is perfectly true that the cattle of various parts of India differ materially in size. Several of the sheep and goats of the mountains are large and portly animals, and the domestic breeds are used, the sheep especially, for carrying loads; whilst the wild species, as the *bharal* or argali, and the *jamlah* goat, are, according to all accounts, animals of great size and strength. The *dumba*, or broad-tailed sheep, is common in central Asia. In Bokhara[s] all the sheep are of this kind; some of the tails yield as much as fifteen pounds of tallow. The animal looks deformed from its size, and straddles along with evident uneasiness. Some of the horned cattle of the northern provinces of India are tall and handsome animals, whilst the forests produce the two large species of the Gaur and the Gayal, and the buffalo itself appears to have been known to the ancients as the Indian ox. The Turkman horse is celebrated for size and strength. On the other hand, the animals of the more southern parts of India are of a diminutive breed. The horses of Bengal rarely exceed thirteen hands, and the goats and sheep are of the smallest size. The zebu, or hunch-backed Indian cow, is not taller than an English calf; and there are still smaller breeds, some of which are not so large as a moderate-sized mastiff dog. With regard to the absence of the hog, we must here agree with Aristotle, that Ctesias is not worthy of belief, for both the wild and domestic kinds are known to the Sanscrit language, and the medicinal properties of the flesh of both kinds are described by Hindu writers. It is to be observed, however, that Ælian[t], who repeats the remark, adds, that the Indians abhor the flesh, and would as soon feed on that of man; a very superfluous disgust, if the animal was not known to them. It is quite true that no Hindus, except the outcastes, will eat the flesh of the *Gráma súkara*, *Gaon ka súr*, the village or tame swine; but the wild hog is permitted food, at least to persons of the military caste; and I recollect partaking of one which had been sent as a present from the raja of Bharatpur to the English officer commanding at Mathura. The wild hog was indeed an animal high in esteem amongst the Hindus in former times. The flesh of the wild boar offered to deceased progenitors, satisfies them for ten months[u], and to the goddess Káli, satisfies her for twelve years[x]. One of the principal avatars of Vishnu is denominated the Varáha, or boar, in which the deity, assuming the head of the animal, upraised upon his tusks the earth which had been submerged beneath the ocean; as in the concluding passage of the drama, Mudra Rakshasa, it is said of the earth,

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[r] Ælian iv. 26.

[s] Burnes, ii. 174.

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[t] Lib. xvi. c. 36.

[u] Menu, iii. 270.

[x] Asiatic Researches, vol. v.

> That erst, by power divine upborne, she rode
> Safe on the tusks of that celestial boar,
> Who snatch'd her from the o'er incumbent floods,
> And rear'd her green hills once again to heaven[y].

XI. .IV. 26. Of hares and foxes it is asserted, "that they are "hunted, not with dogs, but with crows, hawks, "kites, and eagles;" and the statement is so far true, that in India, properly so called, dogs have never been associated with man in the chase. Hawking has long been common, both with Mohammedans and Hindus; and in native portraits of Nawabs and Rajas, it is very common to see the hawk sitting on the wrist of the individual who is delineated. It is doubtful, however, if the practice was not introduced by the Mohammedans; for there is no trace of this amusement in any of the Sanscrit poems or plays.

XXV. .IV. 25. "India has wild asses as large as horses, with a "purple head, blue eyes, and a white body; they "have a horn on the forehead a cubit long, the "lower part of which, or that nearest to the skull, "is very white, whilst the middle is black, and the "tip red: raspings of this horn infused in any fluid "are a preservative against poison, and either water or wine drank out of cups made of it, either "before or after swallowing poison, will prove an "antidote: drinking from such cups is also a protection against spasms and epilepsy.

"All other asses, whether wild or tame, and all "animals with solid hoofs, are without astragali and "without gall, but these animals have both." The bones which Ctesias says he had seen, he describes "as very handsome, they resembled the astragali of "the ox in form and size, but were as heavy as "lead, and of the colour of minium, externally and "internally."

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[y] Hindu Theatre, 2. 152.

"The Indian wild ass is a very strong and swift "animal; no other can overtake it. At first it runs "gently, but it increases its velocity as it proceeds. "It is captured only by this device:—when droves "of them take out their young to pasture, they are "surrounded by horsemen, and being unwilling to "abandon their young, they do not gallop off, but "remain and fight with horns, heels, and teeth, "and kill many of their assailants: at last, however, they are overpowered and slain with arrows "and darts; they are never taken alive. They are "hunted for their horns and astragali, for the flesh "is too bitter to be eaten."

In this account there is an evident confusion of right and wrong, and the circumstances obviously relate to two different animals, the rhinoceros, and the wild ass or horse; the former of which is really an Indian animal, whilst both the latter are numerous in the countries immediately north of the Himalaya.

The peculiarity of the horn, and the belief in its virtues as an antidote, belong to the rhinoceros. The same powers are generally ascribed by the orientals to cups made of it, as Chardin, speaking of the popular notion that prevailed in his day, observes; "On prétend qu'aux Indes les rois et les princes se "servent de cornes de rhinocéros à boire, à cause de "l'antipathie qu'elle a avec le poison lequel se re-"connoit en ce que le corne sue au moindre poison "qu'il y a dedans." A similar statement is given in Wilkins's edition of Richardson's Dictionary. In neither the Heft Kulzum nor the Burhan Katia is this property mentioned, although they both state that raspings of the horn taken internally expel wind. The Makhzan al Adwíah, a medical work of great merit, notices that drinking cups are made of the horn of the rhinoceros; and Baber[z] relates that he had a goblet made from one: a belief in the alexipharmic virtues of the material was no doubt the reason for the fabrication of such vessels, and the notion seems to be very widely diffused throughout the East. Thunberg, at the Cape of Good Hope, observes, with regard to the horn of the African rhinoceros, it was esteemed for making drinking cups, as any poisoned liquor in them would ferment: he even submitted this to the test of experiment; but found, as might have been expected, the bowl was very unconscious of its drugged contents. In Persia we have seen that the idea prevailed in the time of Chardin, and one cannot doubt its currency in India. Buffon mentions various authorities to this effect. Mr. Forbes, speaking of the notion at the Cape, says it is as common there as in India; and in a little treatise on natural history, printed at Serampore, in Bengali, partly translated and partly original, it is said the Hindu physicians consider the horn, teeth, nails, skin, and other parts of the rhinoceros, as eminently medicinal in a variety of complaints, and that Indian princes have drinking cups made of the horn, on account of their being antidotes to poison. The same belief extends to the eastern Archipelago and to China, and it has been said, that to the latter, a thousand rhinoceros' horns are annually exported from Siam [a]. The horns are employed by the Chinese for supposed medicinal purposes. So far, therefore, little doubt can be entertained that the one-horned animal of Ctesias is the rhinoceros.

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[z] Memoirs of Baber, 316.

The Indian, or rather the Persian name of an animal, probably the same as the unicorn of our text, which also occurs in Ælian [b], identifies it with the rhinoceros. It is there termed *kartazon*, or *kartazonos*, καρταζωνος, and this has been corrected with very good reason, by Bochart [c], to *karkazon;* for the Persian name of the rhinoceros is *kargadan*, كرگدن ; or as it is also read, according to the Burhan Katia, كرگزن *kargazan;* both of them connected apparently with the Sanscrit appellation *kharga*. This in Bengali is confined to the horn, and the animal is in that language and in Hindi, called *Genda;* in this case we might have the compound, *s. khargarán*, *p. khargadán*, the original of the *kargadan* of the latter. Tychsen, adhering to the common reading, would explain it by كرك *a rhinoceros*, and تازان *running*, as if it meant the fleet rhinoceros: the compound is not known to the Persians; and whatever may be said of the speed of the rhinoceros, no person familiar with the appearance of the animal can associate with it the idea of swiftness. All that is meant probably by the accounts of its velocity, is, that it runs faster than might be expected from its figure: it is easily overtaken by a horseman.

Of the anatomical peculiarities noticed, it is true that many of the solid-footed animals, as the horse, ass, deer, have no gall bladders, and that the rhinoceros has: but the divisions of his toes exclude him from the class of solid-hoofed animals; and here, therefore, the description applies rather to the horse or ass, than to the rhinoceros. Animals with divided as well as undivided hoofs, however, have the tarsal bone, called the *astragalus,* unless Ctesias meant something else by the appellation. What use this bone was applied to, so that the animal should be hunted for it, is not described; but it might have been employed as a sort of dice in some game of chance, or worn as a talisman. According to the Makhzan al Adwíah, rings for the fingers and thumb are made from the horn, and they were possibly made from the heel bone also, to be worn as preservatives against venom. From its red colour, Tychsen supposes it must have been painted; but in that case the colour would have been only superficial; and it is worthy of notice, that Ælian says the colour throughout is 'black.' There is more plausibility in Tychsen's conjecture, that the various colours of the horn applied to it after its formation into a cup, when they were given to it by art; a supposition confirmed by the passage which he cites from Manuel Philo, in which the author asks an Indian prince [e] what the cup out of which he drank was made of, and which was ornamented with three rings, white, black, and red. The reply was, that it was made of the horn of the onager. At the same time it is to be observed that the horn of the rhinoceros appears to undergo a change of colour, and is sometimes grey, approaching to white; and the white is most highly valued.

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[a] Crawford's Embassy to Siam.

[b] xvi. 20.

[c] Hieroz. P. 1. b. iii. c. 26.

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[e] Heeren, ii. 384.

Of the flavour of the flesh it is to be remarked, that Persian physicians declare that the flesh of the horse and the ass is sweet to the taste, but bitter in digestion; whilst the Hindu medical writers affirm that the flesh of the rhinoceros is sweet: what is also more germain to the matter, Manu[f] declares it unlawful to eat the flesh of any animal with undivided hoofs, and enumerates the rhinoceros as amongst the animals that may be eaten. The Makhzan al Adwíah observes, also, that the flesh of the rhinoceros is halál, *permitted, good;* and the Bengali treatise, above referred to, states that the Hindus eat it. Here, therefore, Ctesias means the horse or ass.

Besides the singleness of the hoof and qualities of the flesh, the figure, size, and speed evidently refer to an animal of the family of the horse, and either to the wild ass, the Gor khar, or to the wild horse, the dzigtai or equus hemionus, or some other species, for both are common in the regions north of the Hindu Koh: the Gor khar, however, must have been well known in Persia, where it is still, as it ever has been, native, and a favourite object of the chase; and the wild horse is therefore more probably the animal intended. This, as found under the name of Kiang, in Ladakh, by Messrs. Moorcroft and Trebeck, was white about the nose, under the neck, and in the belly and legs; the back was a light bay, and the mane was dun: they herded in considerable droves, and the manners of the animal were such as Ctesias describes: at first he trots off gently, when a pursuer approaches, stops and turns, and looks at the object of his fear, and then flies off with unapproachable speed and irreclaimable wildness. They are occasionally, but with difficulty, shot, but never taken alive. It seems most likely, therefore, that this was the animal of whom Ctesias received accounts; but either he or his informants blended them with some imperfect description of the rhinoceros. The compound seems to have been in some respects a mythological one, as the sculptures at Persepolis contain several representations of the unicorn, with the body, according to Ker Porter[g], of a bull, but according to Niebuhr and Morier, of a horse.

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[f] v. 11. and 18.

III. Birds. There is a chapter on gallinaceous fowls of the largest size, of which the title only is preserved. Some of the particulars, however, seem to be given by Ælian[h], who describes the birds as having many-coloured crests, broad tails, feathers and plumage of partly a golden and partly an emerald colour. Ctesias might have alluded to the peacock, which was not known to the Greeks, apparently, before Alexander. Cuvier considers the bird described by Ælian to be the Impeyan pheasant (Lophophorus refulgens).

III. The parrot is well enough described[i] as "being of "the size of a crow, with a red beak, a black head, "and a blue or a red neck, with human voice and "speech, and talking either Indian or Greek, accord-"ingly as it was taught." The name of this bird, which, although it is read much more frequently Psittakus, Ctesias calls Bittakos, is derived by Reland from تدک, Tadak, one of the synonymes of a parrot in the Persian language. On this Tychsen remarks, "From this might come τεδακος, τιδακος, "σιττακος, and finally ψιττακος; and it is just possible that Ctesias might have written the name "Tedek, with a ب *b* instead of a ت, thus changing "it to Bittakos: but this," he adds, "is mere conjecture." And it is too much in the old style of etymological license to deserve much weight. The more usual sense of Tadak also is *heathcock*, and the current name of *parrot* is Tuti. The Sanscrit name Suka, or Sukas, furnishes some of the elements of Psittakus; and there is some approach to the latter in one of the generic names of a bird in Sanscrit, *Pitsat*, "habitually alighting." But no great stress need be laid on these coincidences. The conversational powers of the parrot have long been famous in India, though it should appear that they were novelties in Persia in our author's day. There is now, however, a popular collection of tales in the Persian language, supposed to be narrated by a parrot, the *Tuti nama*; the idea of which was perhaps suggested by a similar series in Sanscrit, the *Suka Saptati*, "the seventy stories of a parrot." The parrot and other birds, as the Maina or Grakle, and Koïl or cuckoo, have always been pets; as in the Mrichchakati: "The pampered parrot croaks like "a learned Brahman, stuffed with curds and rice, "chaunting a hymn from the Vedas; the maina "chatters as glibly as a lady's maid repeating her "mistress's pleasure to her fellow-servants; and the "koïl, crammed with juicy fruit, wheezes like a "water carrier[k]."

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[g] Heeren, i. 154.

[h] Æl. xvi. 2.

[i] Pliny. x. 42. Suidas in voce ψιττακος Olympiodori Excerpta apud Phot. Diod. Sic. 2.

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[k] Hindu Theatre, i. 86.

xvii. . IV. 41. "There is also, it is said, a bird called Dikærus, "the meaning of which in Greek is *δικαιος*. It is not "larger than the egg of a partridge, and is remarkable for concealing its dung, on purpose that it may "not be found, because the substance is of a poisonous "nature, and if as much as a sesamum seed of it be "swallowed in the morning by any person, he falls "into a sound sleep, and expires by sunset." Reland confesses he can make nothing of the name of this bird; and his conjectural derivation of it from the Persian دلمك, *Dilmek*, a venomous spider, is, as Tychsen observes, rather farfetched. The same, however, may be asserted of the etymology which he would substitute of دي, *Di*, "good," and كار, *Kár*, "maker," i. e. benefactor. If we might suppose an error in one of the letters of this word, and read it Ditæros instead of Dikæros, we might find a Sanscrit original for it in Tittiri, *a partridge*; or, if the initial were changed to a B or V, in Vikira, *a bird in general*. But these also are mere conjectures. Ornithologists have agreed to apply the name Dicæus to a class of small birds, of the family of creepers, which exists in India, the habits of which are not known, nor am I aware of the native name. According to Ælian, the colour is that of Sandarach, or red, which would apply to the little Amadavat finch. The effects of the dung, as described by that writer, identify it almost to a certainty with opium, small pills of which may have been mistaken for it, or have been metaphorically so termed. The death it induces is, according to him, a gentle dissolution, a sweet sleep. It was therefore regarded by the Persian monarch as the most acceptable present which the Indian sovereign could send him, as an agreeable refuge, in case of necessity, from irremediable ills.

XVI. Reptiles.

Of reptiles, we have an account of "a snake nine "or ten inches long, of a purple colour, with a very "white head, and without teeth, which is met with "in the same mountains where the sardonyx is ob-"tained. It cannot bite, but any part upon which "its saliva falls mortifies. When suspended by the "tail it emits two kinds of venom; one whilst "alive, which is black, and like a sort of amber; "another after it is dead, which also is black. A "sesamum seed of the former kills immediately, "and the brain of the person comes away through "the nostrils. The latter brings on a gradual de-"cay, and proves fatal in about a year."

It may not be possible to identify the reptile here alluded to, but there are various small snakes in India to which deadly properties are ascribed. Several of the kinds of Typhlops, or blind snakes, are not larger than common earth-worms, being not above nine inches long, and not thicker than a crow-quill[l]. The bite of some of these is said to be fatal, whilst that of others produces extreme debility. Of one small species, Mr. Forbes[m] relates that its body is covered with a glossy slime, which it leaves like a snail wherever it crawls, and which is believed to be poisonous. Its teeth were visible, according to Mr. Forbes, only through a microscope; but Cuvier[n] says he had not been able to discover any teeth in those which he had examined.

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[l] Russell's Indian Serpents, plates 36, 37, 42, 43.

[m] Oriental Memoirs, i. 229.

[n] Griffiths' Animal Kingdom, ix. 248.

The saliva, as well as other secretions of the snake, and of some other reptiles, is considered to be poisonous; and the poison of the snake is collected, and administered internally, for medicinal and deleterious purposes. In the poison described by our author, however, we might again suspect traces of opium, not only in its black colour and its effects, but in its concurrence with the sense of the Sanscrit name of opium, which is *Ahiphena*, meaning the foam, froth, or saliva of a snake. The history of this word, however, is uncertain, and instead of being the parent, it may be the offspring of οπιον (opium), through the Arabic *afiun*.

XXI.
El. IV. 46. "There are in India insects of the size of a "beetle, of the colour of minium, having very long "feet, and as soft as worms. They are found on "the trees which bear amber, and feed upon and "spoil the fruit in the same manner as the phthires "do grapes. The Indians, by bruising these animals, obtain a dye, with which they dye robes and "tunics, and other articles of dress, of a scarlet "colour, very superior to the Persian dyes."

Although the description of the insect is not perhaps very exact, yet the remarkable peculiarity of its affording a scarlet dye identifies it either with the cochineal or the lac insect. Wilford[o] supposed it to be the latter; and its connexion with amber, which will presently be adverted to, or, in other words, with shel-lac, renders it probable that the lac insect is intended. Beckmann imagines it to be the cochineal, but Heeren[p] leans to the lac: he observes, "A third and no less certain class of pro-

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[o] Asiatic Researches, ix. 65.

[p] ii. 209.

“ductions which the Persians and Babylonians obtained from India, were dyes, and amongst them “the cochineal, or rather Indian lacca.” He infers also the domestic use of this dye in the colouring of stuffs, which were likewise imported, and which were nothing else than Cashmir shawls. Mr. Royle[q], in his Illustrations of the Botany of Northern India, conceives that the lac insect cannot exist both in the sultry plains of India and the cold and arid table-land of Tartary, to which Heeren refers it, and to which, or at least to Tibet, the locality of Ctesias applies. Admitting this to be the case, and that the insect in question was not the coccus lacciferus, a kind of cochineal is found on the roots of a plant in a marsh near Herat; and a similar insect, probably the kermes, is an article of considerable import at Herat from Bokhara and Yarkand, being brought originally either from Russia or China. Lieut. Burnes also describes a worm which attaches itself to the root of a plant growing luxuriantly on the banks of the Oxus, and other rivers in the same country, which produces a purple dye like that of cochineal. He says he compared it with American cochineal, and they appeared to be similar, only that the native preparation was softer. The people however do not well understand the management of the insect, although, in their imperfect process, it still produces a dye inferior only to cochineal[r]. It might therefore have been this insect, perhaps a kind of kermes, which is described by Ctesias as the produce of northern India, if it was not the lac

[q] Botany of the Himalaya Mountains, p. 85 and 225

[r] Travels to Bokhara, ii. 169.

insect: but that this latter does not live in that direction is yet to be determined. It appears that this is not the only question that is doubtful regarding the lac insect; as in Griffiths' Animal Kingdom it is said that there is no very positive proof that the lac which is brought from India is owing to a cochineal. The description given of the insect by Dr. Roxburgh[s] conforms generally with those which have been published of the cochineal insect, though the plants upon which it is found embrace a greater variety, and the construction of its nest may differ. The name is Sanscrit, implying a multitude, from लक्ष, "a lac—a hundred thousand," in reference to the number of small insects found in a nest. And the denomination of *kermes* is also of Indian origin. Cochineal is called by the Persians *kirmiz*, from *kirm*, کرم, Sanscrit *krimi*, कृमि, a worm or insect.

I. "The only animal which is found in the Indus is
XXVII. "said to be a kind of worm (σκωληξ), of the same
El. V. 3. "kind as that which is found in the fig-tree, varying in length, but commonly seven cubits long, "and so thick that a child of ten years of age cannot grasp it. It has two teeth, one above, one "below, and with them devours whatever comes "within its reach. During the day, it burrows in "the mud, but at night it issues on the land, and "carries off oxen, or even camels. It is taken with "a large hook, to which a goat or sheep is fastened "with an iron chain. When captured, it is hung "up for a month, with vessels placed underneath, "into which runs as much oil as would fill ten "Attic cotylæ (or nearly five pints). The carcass is "thrown away, and the oil is sent to the king, no

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[s] Asiatic Researches, ii. 360.

"other person being allowed to possess it. This "oil burns whatever it touches, consuming living "beings as well as timber and the like; and it is "only to be extinguished with soft clay."

In this reptile we have, no doubt, some imperfect accounts of the size, habits, and rapacity of the alligator; whilst, in some respects, the scolex may be no more than the hydrus, or water-snake. The flesh of the alligator is said by native physicians to be greasy, but I am not aware of any oil being extracted from it: at the same time, it may be practised in some parts of India. In an alligator, at the dissection of which I assisted, enormous masses of yellow fat were found under the skin, which might have been converted into oil. A coarse oil extracted from fish (matsya tailam) is in common use in India for different purposes: and the oily fat of the porpoise (Delphinus Gangeticus), which is probably an inhabitant of the Indus, is also considered as an external application of great efficacy for pains of various kinds. The flesh of the lion is also boiled down for its oil. For inflammable oils we have bhumi tailam (earth oil), or petroleum, and silajit, or bitumen, which is supposed to exude from rocks. Ælian adds, that the substance in question is used in sieges; and an earthen jar containing the oil being thrown against the gate of a town, the jar breaks, and its contents, taking fire, consume the place and its defenders. The Hindus appear to have had a knowledge of some kind of fire-arms in ancient times; and the Sataghni, or 'destroyer of a hundred,' is commonly described as a sort of rocket, a weapon which was generally used in Indian armies at a later date.

xiii. lants. We have only the heading of a chapter on the Indian palm, with the remark that the fruit is thrice as large as the Babylonian.

xix. "There are trees in the mountain regions growing "in the water, which at certain periods of the year "drop tears, like the almond or pine, or the like, and "this annually for thirty days; and these tears, falling into the water, become congealed. This tree is "called, in the Indian language, *siptakhora*, which "signifies in Greek, *sweet*, *agreeable* (γλυκυ, ηδυ), "and the Indians gather amber from it. The fruit "of some of these trees grows in clusters like grapes; "the kernels are as large as the nuts of Pontus."

There is nothing uncommon in India in the exudation of tears, or of a gum or resin, from a tree: and many valuable products of this nature, as olibanum, bdellium, mastich, kopal, katechu, and the like, are obtained from trees of Indian growth. The last-named resin is the produce of a tree, of which the name affords some resemblance to Sipta khora, the Sanscrit Sweta khadira, श्वेतखदिर, or, in the spoken dialects, Set-khair: but Sweta or Seta, श्वेत, means *white*, not *sweet*, which in Sanscrit is Swádu स्वादु. Reland and Tychsen agree in supposing khora to be derived, as in the case of Martikhora, from the Persian Khorden, *to eat;* and the former refers the first member of the compound to the word Sefa, صفا, *pure*, *pleasing*, which, as an Arabic, not even a Persian word, is quite inadmissible. Tychsen deduces it from Shifteh, شیفته, asserting that Shifteh khor means *agreeable to eat.* Shefteh, Tychsen says, comes from Sheften, which means *to be in love;* and Shaftarang, Shaftalu are applied to the peach and apricot (or rather the nectarine), as if, according to Wilkins, the fruit was of the colour of love, or was the apple of love. In the Burhani Katia, however, a well of Persian undefiled, one of the senses of Sheften is چکیزشن, *to ooze, to drop;* and this will agree well enough with the properties of the Sipta-chora tree. But it is said of this tree, that the Kalystrii, the Indians of the mountains, feed upon its fruit, and dry it like grapes, and make it an article of barter. Now this applies especially to the apricot, which appears to be in its native land in Tibet and the countries beyond the Hindu Koh, and is dried and exported in considerable quantities. At Kaubul, according to Mr. Royle, it is said to be preserved in fourteen different ways. At Balkh[t] it is so plentiful that 2000 are purchasable for a rupee, or two shillings. In the Himalayas the name of the fruit is Zard-alu Chulu, and Chinaru. It is imported into northern India under the Persian name Khubaní, or, *very good.* The manner of its growth does not accord with that described by Ctesias; but we must not expect to make every particular accord.

According to Mr. Royle[u], the peach, apricot, cherry, and plum, all exude gum in northern India; and in this we have the amber of the Sipta-chora. But here is probably some of that confusion which evidently pervades several of our author's descriptions, the blending into one article circumstances which are true of two or more. It is said of this tree, that the insects which furnish the red dye live upon it, and if that be the lac insect, the amber is probably nothing more than the substance of its nest, the resinous walls of the cells in which it breeds; in regard to which Dr. Roxburgh remarks, that it cannot be better described with respect to appearance than by saying it is "like "the transparent amber that beads are made of[x]." It may be some further illustration of Ctesias's account, that the inhabitants of the countries where his amber-dropping tree is said to grow, are still particularly fond of wearing beads of amber, or of some similar resin. They procure it through the Punjab and Caubul chiefly, but from what source is not known.

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[t] Burnes, ii. 171.

[u] Illustrations of the Botany of the Himalaya.

But we have still another tree, and this in the mountain which may have something to do with the Siptakhora, especially as the name Swadu-kola, or Swetacola, a sweet or light-coloured variety of the Badari or jujube might be applied to it. This is common in the jungles of northern India, according to Royle, where the fruit is dried and pounded. This kind also bears a kind of lac, which is used for dying leather, cotton, and silk[y]. And in the fruit and gum, as well as the name, therefore, a tolerably close approximation occurs. At the same time it seems most likely that the apricot furnished the principal features of the Siptachora, mixed up with others that belong to different trees. In fact the word Siptachoras, as he himself explains it, meaning merely sweet, or as Pliny[z] understands it, *prædulcis suavitas*, is applicable to any tree bearing sweet fruit.

[x] Asiatic Researches, vol. ii.

[y] Illustrations of the Botany of India, p. 170.

[z] Lib. xxxvii. 11.

“ At the source of the Hyparchus river grows a XXI.
“ purple flower which yields a purple dye no way
“ inferior to that in use amongst the Greeks: it is
“ rather brighter.”

If we are to understand by purple (πορφυρα) a blue colour, the indigo plant readily presents itself. This grows as well in northern as in central India according to Mr. Royle, and lieutenant Burnes found a bastard species of it in the neighbourhood of Bokhara, although the true plant, he says, does not grow there. Its use, as a dye, was long known to the Hindus, as in the Pancha tantra, the collection of apologues, which is the original of Pilpay's fables[a], and which is known to be of a date anterior to the sixth century at latest, there is a fable of a jackal having tumbled into an indigo vat. The north-west of India, however, is rich in dyeing drugs, and the Rubia Manjith, or Madder, or the Kusumbha, *Carthamus tinctorius*, both growing abundantly beyond the Himalaya, might be the plant in question; the former yielding a deep red or crimson dye, and the flowers of the latter being employed for a rose-coloured, purple, and violet dye.

“ The Indians have trees as tall as the cypress or XXVIII.
“ cedar, whose leaves are broader than those of the
“ palm, but like them in other respects, and without
“ any axillæ. They bear a flower like that of the
“ male laurel, but no fruit. The Indian name is
“ Karpion, the Greek Myrrho-rhoda. They are not
“ common. An oil exudes from them, which is
“ caught by flocks of wool fastened to the tree, and
“ thence transferred to alabaster vases. It is of a

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[a] Transactions of the Royal Asiatic Society, vol. i. p. 164. Hitopadesa, b. iii.

"reddish colour and viscid consistence, but so fragrant that its odour extends for five stadia. It is given only to the king and those about him. The king of India made a present of some to the king of Persia, and Ctesias says he saw it, and the smell was such as he could not describe, except by saying it was unlike any other."

The fact of a fragrant oil, or perhaps rather of a fragrant paste, having been sent from India to Persia, can scarcely be questioned. This could not have been *atr* of roses, not only from the description of the tree, but from the peculiarity of the odour. Had that been the fragrance of roses, Ctesias need not have been at a loss to describe it. The perfume to which the account of it best conforms is that of Sandal, which is very generally used in India, both as a paste and a viscid oil, and is obtained from the wood of a handsome tree, although it does not resemble a palm in its foliage. The name *Karpion*, however, does not belong to the sandal tree, nor to any other Indian plant, as far as I have been able to ascertain. The nearest approach to it is the Sanscrit Karpura, which, through the Arabic modification Kafur, has supplied the European term *camphor*: an oil of camphor is extracted from the tree, and well known in India, and the drug is obtained from a species of laurel, according to Kæmpfer[b], as well as from a variety of trees not so properly the growth of India as of the countries to the eastward, or Sumatra and Malacca. This might have found its way through India to Persia; but, unless mixed with some other substance, would not have been of a reddish tint. Nor is the etymological affinity very satisfactory, although it is fully as much so as any other proposed. Reland derives Karpion from the Arabic name of cinnamon, قرفه, which, however, is a derivative from قرف, *decorticavit,* and means the peeled bark, not the tree. Tychsen agrees in supposing the latter to be meant, on the authority of Paulinus a Bartholomæo, who asserts that the Sanscrit name of cinnamon is Karuva, but no such Sanscrit word occurs in medical vocabularies, and it is more properly the Tamil term, Kárruwá. I am not aware, however, that an oil of cinnamon is known in India.

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[b] Asiatic Researches, vol. xii.

XVIII. "There is a sort of wood (or rather a tree) called " Parēbon, or Parubon (παρηβον, παρυβον). It is of the " size of the olive, and is found only in regal " gardens. It bears neither flowers nor fruit, but has " fifteen roots under ground, each of the thickness of " a man's arm. A piece of this, of a span length, " attracts every thing to which it is directed, gold, " silver, brass, stone, and all substances except amber. " A piece of a cubit in length will attract even sheep " and birds, and it is employed especially to catch " the latter. If you wish to congeal water or wine, " put three oboli of the root into a gallon of fluid, " and it will become like wax, so that it may be " held in the hand. It liquefies again in the course " of a day. It is of benefit in cholic."

The wonders related of this shrub, or wood, have originated perhaps in the properties of some plant with a viscid or glutinous juice, to which light substances may adhere, and which mixed with fluids forms a strong mucilage and demulcent. Many of the Malvaceæ and Tiliaceæ, affording mucilaginous juices, are found in India, and a strong mucilage used as a demulcent is prepared from the seeds of the holy Basil. The employment of Lásá, لاسا or bird-lime, to catch birds, is well known in India, and it is obtained from various plants: amongst others, from the stem of the Indian and the religious Fig. Reland has not made any attempt to explain the origin of the name Pariban, which Tychsen resolves into the Persian compound Bár-awar بار اور, "load-bringing." The first part of the term may be referred to the Sanscrit *pari*, परि, "about," "back "again," and the latter to *vana*, "a grove:" *pari-vana*, "in or about a grove, or garden:" but there is no authority for such a compound, although *pâri* occurs as part of the appellation of different trees, as *pári-jata*, "the coral-tree," *paribhadra*, "the silk-"cotton."

VI. "In the plains and mountains through which the "Indus runs there grows the Indian reed. This varies "in dimension, some being larger and some smaller, "but there are some so thick that two men with their "arms extended cannot encircle them, and some are "as lofty as the mast of a merchant vessel. These "reeds are male and female, and the female has a "sort of medulla which the male has not. The latter "is the larger."

We need not be at a loss in this account to recognise the Bambu, which grows abundantly in most parts of India, and often attains a considerable size. The height is not at all exaggerated. The circumference exceeds the truth, as the largest kind which is found in Ava and Pegu rarely exceeds twenty inches in girth. Ctesias, however, is less extravagant in this than even Herodotus [c], who asserts that a single joint of the bambu is sufficient to form a canoe. The distinction of the plant as male and female, the latter being so considered from its more slender form and larger cavity, has been preserved in popular language to the present day. By the Enterione (εντεριωνη), the pith or medulla of the female bambu, Ctesias probably intended the peculiar substance found especially in the smaller kind of hill-bambu, long known to Oriental physicians as *tabasheer*, and highly prized as a febrifuge, demulcent, and restorative in Persia, Arabia, and Turkey. This, which is at first a watery fluid, gradually becomes viscid, and finally solid, of an *opal* or milk-blue colour, more or less semi-transparent; whence the latter part of its name *shir*, from *kshir*, "milk." It has been the subject of careful investigation, in the first instance, on account of its supposed medicinal virtues, and latterly for its curious optical properties, and very low refractive power. Dr. Russell has treated of the first in the Philosophical Transactions for 1790, and Sir David Brewster of the latter in the same collection for 1819, as well as in his Journal, November to April, 1828. The latter writer considers it to be nothing more than the sap of the bambu extravasated into the hollow in consequence of a disease in the spongy part of the internodal substance through which it should properly circulate. Humboldt is of opinion that Salmasius has rightly referred the saccharon of Pliny to the medulla of the bambu [d], describing it as a honey collected in canes, of the whiteness of gum, crumbling between the teeth (*dentibus fragile*), in lumps about the size of a filbert, and used only in medicine; concluding from this and other notices found in ancient authors respecting sugar, that hard sugar was unknown to the ancients, and that when they speak of a solid sugar they mean tabasheer, or sharkara of bambu[e]. Unfortunately, however, for this conjecture, the little flavour that tabasheer possesses is rather salt than sweet, and in its dry state, in which alone it is exported, it is so hard that it would be much more likely to break the teeth than to be broken by them. The saccharon of Pliny is much more likely to be manna, which is an Indian as well as an Arabian product. The allusion to the interior secretion of the bambu by our author is valuable as a test of his accuracy, and of the probability of the substance being then, as it has ever since been, an article of imaginary, if not of real medicinal value; the notion being derived from the medical writers of the Hindus, which describe the Bans-lochana, also called Twak-kshiri, whence Tabashir, as nutritious, strengthening, aphrodisiac, sweet (medicinally), and cooling; a remedy for thirst, fever, consumption, asthma, bile, blood, jaundice, and leprosy. "Bháva "Prakás."

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[c] iii. 98.

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[d] xii. 17.

IV. Minerals. There is little more than the heading of a chapter which treats of a fountain of liquid gold[f], "which is drawn off, it is said, in vessels of baked "clay to the extent of a hundred annually, each "holding one talent. The gold hardens as it is "drawn, and it is necessary to break the vessels to "get it out. It is a square fountain, sixteen cubits "in circumference and one in depth."

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[e] Humboldt on the natural family of the Grasses. Quarterly Journal of Science, vol. v. 51. and vol. vii. 363.

[f] Philost. de v. Apollonii 3.

This looks like some imperfect account of a bed or furnace in which gold had been melted or refined, and had been drawn off for casting in earthen crucibles.

XII. Ælian. xxxvii. 4. In another passage it is stated, that "gold is not "obtained in India from the sands of rivers by "washing, as it is from the Pactolus, but is found "in lofty mountains inhabited by griffins. These "are birds with four legs, of the size of a wolf, "with the legs and claws of a lion, having red "feathers on the breast, but black on the rest of "the body. It is difficult to procure the gold on "account of these birds."

Although it is not true that gold is not found in the sands of rivers in India, as those of many of the hill streams of the Himalaya [g], and those of the Oxus are washed for the ore, yet it is found native in various parts to the north of the Himalayan chain, as in the country of the Kafirs and in Tibet [h], and is sometimes met with in considerable lumps. In the latter country a popular prejudice, and even the orders of the government, are adverse to its collection; for it is believed to be the property of certain spirits, the genii of the place, and the special ministers of the God of wealth, whose anger is excited by any attempts to despoil them, and, unless appeased by suitable homage, proves fatal to the sacrilegious individual who has taken away the metal. These superstitions may have formed the basis of the stories of griffins and of ants as large as dogs; the former of which is alluded to by our author, and the latter described more particularly by Herodotus, Ælian, and Strabo. The *γρυψ*, *grups*, or *griffin*, of the text, may very probably be connected, as Tychsen supposes, with the Persian verb, گرفتن, *to seize;* whence the German *greifen*, and our words *grip, gripe;* the whole family being related to the Sanscrit *graha, grihitum, to seize*, or *lay hold of.* The anomalies of his form may also be Indian, and borrowed from the Garura, the bird and vehicle of Vishnu, who is commonly represented with the head and wings of an eagle, and the body and limbs either of a man, or a bird of gigantic size. In the latter case he is sometimes represented as carrying an elephant in one claw, and a tortoise in another. A modification of Garuda also occurs in the south of India, in the Gandabharunda, a bird with four claws, each holding an elephant. There is also a bird called in Menu, Hema-kára, *gold-maker;* which, according to Wilford, is commonly described as of vast stature, living in the mountains to the north-west of India, whose dung mixing with the sand of the country converts it to gold. A bird that voids gold is mentioned also in the Panchatantra[i]. The belief in the existence of such an animal as Ctesias describes was probably established in Persia when he resided there; for, as Heeren observes, his description so exactly coincides with some of the Persepolitan sculptures, that it would almost appear as if he had seen the relievo, or the artist had copied his description[k]. With regard to the ants, although they are the creatures of other narrations, it may be observed, that some vestige of the notion may be discovered in Hindu fable and popular belief. All hidden or subterraneous treasure is conceived to be

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[g] Burnes, vol. ii. p. 165.

[h] Ibid, vol. ii. p. 211.

[i] Transactions of the Royal Asiatic Society, vol. i. p. 179.

[k] Heeren, vol. i. p. 183.

under the especial protection of the Nágas, the snake gods inhabiting Pátála, and who commonly appear as serpents, though they may take what form they please. An interesting instance of this belief is given by Mr. Forbes[l], where, in searching a deserted building for treasure, his people found a large snake where they expected money. But the circumstance that connects this with the story of the Greek writers is, that these snakes, and their charge, are very usually found in *Ant-hills*, in the mounds thrown up by the Termites. Thus in the story of the snake and Brahman's son in the Panchatantra[m], the Brahman having learnt the fact in a dream, finds and worships a hooded-snake, whose abode is an ant-hill; and who, in consequence, gives him money and a rich jewel. There is, therefore, an ancient connexion in Hindu belief, between the Termites and buried treasure; and it is possibly from this source that the ants of western fiction have been derived and embellished. The size Wilford[n] supposes to have originated in a verbal error, confounding the Hindu name of an ant "chyunta," چیونٹا, with that of the hunting leopard chíta, چیتا. This is not impossible; but perhaps it proceeded from some perplexed account of the nest of the white ant. Small as the animal is, it throws up burrows of considerable dimensions. I have seen a field strewed with ant-hills as big as moderately sized haycocks, and they are met with larger. This may have been represented as the ant itself, as big as a fox or a Hyrcanian dog.

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[l] Oriental Memoirs, vol. ii. p. 21.

[m] Transactions of the Royal Asiatic Society, vol. i. p. 176.

[n] Asiatic Researches, vol. xiv.

XI. "India, it is said, abounds with silver; and mines "are worked there, although they are not so deep "as those of Bactria." There are no silver mines worked in either country now, and it is not known if the metal exists.

IV. "At the bottom of the gold fountain, previously "noticed, Ctesias says, that iron is found, of which "two swords had been fabricated, and given him by "the king and queen-mother." It is to be inferred, that some more than ordinary value was attached to the stuff of which these swords were made; and it is interesting to find, that Indian steel, Foulad, or Wootz, is still highly prized in the Persian market for the same purpose. The greater part of that which is manufactured in the peninsula, in Hyderabad, or Golconda, is conveyed to Persia[o] by agents from that country, who monopolize the whole of the fabric. The steel is used in the formation of various articles of hardware, but especially in that of the celebrated Damascus or Persian sabre.

Ctesias tells us also that he witnessed the property possessed by rods of the same iron, of averting storms and dissipating heavy clouds, when set upright in the earth. It would seem as if the Persians had some notion of electrical conductors; but the marvel is probably of easy solution; and the clouds passed over, and the storm dispersed, without being materially affected by the iron rods.

V. A chapter contained an account of the great mountains, where the sardo, onyx, and other precious stones were found.

Heeren is of opinion that this refers to the mountains of the Karakorum range, and the country of Badakhshan, where the lapis lazuli is still found in the greatest perfection. In the section, however, treating of the snake whose saliva is so venomous, it is said to be met with in the mountains which produce the sardonyx, and these are described as very hot. We must therefore go more to the southward for the minerals here mentioned, and these are found in the mountains of the west of India. Carnelians, agates, and mocha stones, are found on the Narbadda, and form a valuable part of the trade of Cambay [p], to which the onyxes and other stones from the sardonyx mountains of Ptolemy have been transferred from their former emporium, Barygaza, or Baroach. XVI.

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[o] Journal of the Asiatic Society of Bengal, vol. i. p. 245; Buchanan's Travels in Mysore, vol. ii. 19; Heyne's Tracts on India, p. 358; Tavernier's Voyages.

XI. " There is a lake of the circumference of 800 " stadia, on which, when there is no wind, a certain oily substance floats, which they collect with " buckets. The Indians also use oil and sesamum " from seeds; but that which is taken from the lake " is considered the best."

A variety of oils is extracted by the Hindus from vegetable seeds for culinary purposes, for oiling their hair and persons, and for burning. The only oil that can be identified with that of a lake is the earth oil, or petroleum, which is obtained from wells in Ava [q], floating on the surface of the water. In Cachar [r] a very fine petroleum oil was found floating on water, issuing from the base of a cluster of hills. Something of this kind may occur in the north-west of India, and have been the origin of the lake which Ctesias describes.

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[p] Forbes's Oriental Memoirs, vol. i. pp. 322, 465; and Transactions of the Bombay Society, vol. i.

[q] Crawford's Embassy to Ava, p. 53.

[r] Gleanings of Science, vol. i. p. 283.

XIV. " A river flowing from a rock is said to flow with " honey." This is perhaps a misunderstanding of a figurative appellation. I do not know of any river called in Sanscrit Madhumati, *honey-having;* but it is a legitimate compound; and several streams have the analogous appellation of Ikshumati, or *sugar-flowing, sweet;* in allusion to the fancied sweetness or purity of the water.

XIX. " A river running through India, two stadia broad, " is called by the Indians, Hyparchus, which means, " *bearing all good things.* For thirty days in the " year its waters bring down amber."

This latter circumstance connects it with the Siptachora, or amber shedding tree; and Wilford would consider Hiparchos as nothing more than a corruption of *Sipa-chor*, which again he resolves into *Aspa-cora*, observing that the river was probably thus called because it came from the Sipa-chor, or Aspa-cora country. A town called Aspa-cora is placed obviously, he says, in the country of Tibet, by Ptolemy, and in the Peutingerian tables. The present situation of Aspacora, he observes, is unknown, but he quotes the authority of a native traveller for *gor* and *kar*, being frequently used in combination with other words as the names of places. The river he would identify with the present Tistah. All this however is very unsatisfactory, except the possibility of some connexion between the terms *Sipachora* and *Aswa*, or *Aspacora*.

Reland and Tychsen have not, I conceive, been more happy than Wilford in their etymology of this word. The former derives it from Av, or ap,

او, or اب *water*, and پرخ, perkh, *utility*; a combination as much Greek as Persian. The latter proposes Awarkhosh, اورخوش *the bringer of good*. The Persian form of such a compound would however be Khoshawar. Pliny[s] reads the word Hypobaros, and if his reading be correct, it may be referred to upa-bári, *minor*, or *lesser water*; or su-bári, *good water*.

"There is a fountain of a square form five ells in xxx.
"circumference; the water is in a rock three cubits "above and three ells below the surface. In it the "most respectable persons, men, women, and children, wash; not only for cleanliness, but for health. "Those who bathe in it plunge in, and the water "bears them up. Nothing, indeed, can sink in it, "except iron, copper, silver, and gold, which fall to "the bottom. The water is very cold and sweet, and "rises with a bubbling noise, as if it was boiling in a "caldron. It is beneficial in vertigo and scabies. It "is called Balladee by the Indians, which means in "Greek *οφελειμα, useful*."

There are many mineral springs in India which evolve gaseous matters, but they are in general hot springs. They are very frequent in the hills in Kashmir and Tibet, but their properties have not been investigated. The term Balladee, Reland would derive from Belad, بِلاد, *laudatus*, because what is useful deserves praise. Tychsen traces it to وِلا, or وِلاد, *strong*, *eminent*. He quotes also the authority of father Paulinus, to shew that Ballam is the Sanscrit for *water*, and Nallada, for *good*. These are Tamil, not Sanscrit words. Heeren's translator suggests a much more likely affinity to Balladee in the Sanscrit Baladá, or Baladí, वल्दी, *giver of strength*: and either that or Phaladá, Phaladí,

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[s] Lib xxxvii. 2.

फलदी, *giver of fruit*, or *benefit, beneficial*, is probably the original of the word which Ctesias has preserved.

XVI. "Of another spring it is related, that the water as "soon as it is drawn curdles like cheese, and if as "much as three oboli of it be drunk, the person so "drinking becomes deranged, and relates every thing "he may have done. The king, therefore, employs it "to detect guilt. If the persons accused of any great "crime confess their deeds, (under the influence of "this potion,) they are put to death, if not, they are "set at liberty."

The curdling of the water may have been suggested by a deposit of earthy matter diffused through it whilst it was running, but precipitated when stagnant, like the tufa deposits of many springs in the Himalaya. The application of it, however, is nothing more than the Hindu ordeal, one of their five divine tests of guilt. According to 'the text of Yajnawalkya, the balance, water, fire, poison, and sacred libation, are divine tests for purgation. In the ordeal of water, the accused is thrown into a pond with his hands and feet bound; if he sinks he is innocent, if he floats, guilty. In the ordeal of poison some deleterious drug is administered to the accused with clarified butter. In taking it he recites this prayer: "Thou, oh Poison, art the son "of Brahmá, steadfast in truth, relieve me from "this accusation, and by means of thy virtue become as nectar to me." If after swallowing the draught no change is observable, whilst the hands are clapped together for five hundred times, the accused is acquitted. The clarified butter, the menstruum of the poisonous draught, may be the origin of the curdled water.

The only article that remains to be noticed is the II. marvellous gem the Pántárba, of which all that is recorded is its having recovered from the river, into which they had been thrown by a certain Bactrian pedlar, seventy-seven jewels and precious stones sticking together. It does not appear whether they were thrown in purposely: offerings of precious stones to a sacred stream would have been genuinely Hindu, and there is an indication of an Indian original in the number of the jewels, for seven is the tale of various articles of peculiar sanctity in Hindu mythology, as in the Hanuman Nataka, when Ráma drives his arrow through seven trees, then the seven steeds of the sun, the seven spheres, the seven sages, the seven seas, the seven continents, and the seven mothers of the gods are alarmed. The gem that recovers these valuables has however no certain prototype in Sanscrit, unless it be intended for nothing more than it evidently is understood to be by Philostratus, (iii. 45,) a magnet. That was well known to the Hindus as the akarsha prastara, *the attracting stone*, or the ayas kánta, *the aimant, the beloved of iron*, as in the Málati and Mádhava, the lover says of his mistress; "She at the very first glance attracted my "heart as a rod of the magnet-gem draws the iron "metal towards it." It is not said that Pantarba is an Indian word, although it may not be Greek. Reland has not noticed it: and Tychsen's etymology بند در بياي, *a binding*, or *attraction, underneath;* that is, below the water, is sufficiently improbable. The last part of the word offers some affinity to ruba, ربا, *stealing, robbing*, as in Dil-ruba دلربا, *heart-stealing.* For the first part we might suggest panj, or panjah, *five*, or *fifty*, connected with the

Sanscrit pancha, and the Greek πέντε. The word may possibly, however, be nothing more than a perversion of ahan-ruba, the *iron-stealer*, the *magnet*.

I have thus completed the view which I purposed to take of the narratives that Ctesias has transmitted to us concerning India. The examination to which his statements have been submitted leaves, no doubt, much that is as inexplicable and as impossible as ever; but it will be perhaps admitted to be as satisfactory as any by which it has been preceded, and to be upon the whole favourable to the reputation of the Greek writer. Whatever may be thought of the individual instances in which an attempt at verification has been made, it will probably be conceded that the general character of the story indicates an Indian original, and that the account is not entirely the audacious invention of Grecian mendacity; that amidst much extravagance and incredibility there do appear glimpses of truth; that the description does seem to have been compiled from information, which, however loose and erroneous, was gleaned by actual travel and observation; and that it presents to us a picture of India, in which, however strange in themselves, or distorted by ignorance and credulity, there are authentic features of the country and its natural productions; and of its people, their usages, their arts, their superstition, and their language: and, consequently, that the authority of Ctesias is of value, as evidence of the existence of many of the characteristic peculiarities of India and the Hindus, such as we still find them, two thousand two hundred years ago.

ON

THE AMPHITHEATRE

AT

POLA

IN

ISTRIA.

BY

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ON THE

AMPHITHEATRE AT POLA

IN

ISTRIA.

READ TO THE ASHMOLEAN SOCIETY, MARCH 4, 1836.

THE town of Pola, where the amphitheatre, which is the subject of the present memoir, still arrests the admiration of the traveller, is situated almost at the extreme southern point of Istria, about sixteen German miles distant from Trieste. The bay, whose waters bathe the walls of the town, opens to the sea towards the west, but the islands of Brioni, which lie immediately in the entrance, give it the appearance of an inland lake. The amphitheatre is situated without the town, being built on the side of a hill gently sloping towards the bay, which approaches so near its walls, that the figure of the building is reflected on the surface of the water. It well deserves the panegyrics of Stuart and of Allason, who pronounce it one of the "most striking, beautiful, and perfect monuments of antiquity." Its exterior circuit is nearly entire, but the interior is so utterly destroyed, that Maffei mistook it for a theatre, and Lipsius describes it in these words: *At interiora omnia membra, subsellia, aditus, gra-*

dus, podia e ligno fuisse certum est et pro tempore construi potuisse, destruique[a].

The striking effect produced by this ruin on persons who have already visited the other remains of Roman amphitheatres, is perhaps to be attributed to this circumstance, that we have here no heavy unharmonious mass of ruins in the interior, but the exterior stands forth unincumbered in bold relief against the sky. There is also a peculiarity in the whiteness of the Istrian stone, which even extorted admiration from the envious Maffei. Istria, as is well known, was the quarry whence the proud palaces of Venice were reared, and the stone from this neighbourhood appears to have been of the very best quality. Its hardness has defied the action of the weather; its whiteness almost rivals the polished marble of Carrara, and when struck, it rings like the volcanic tufa, of which many of the buildings at Pompeii are constructed.

It is not the hand of time alone that has spoiled these magnificent structures. Wanton spoliation, or the necessary occupation of them as fortresses in war, has chiefly wrought their destruction. The Italian amphitheatres have in general suffered from both these causes, the French from the former only. Pola likewise has fortunately, to all appearances, been only exposed to the ravages of peace[b], and its interior, as more vulnerable, has totally fallen a prey to the spoilers. Maffei mentions a curious manuscript, of the date of 1303, A. D., in which it is stated of the Patriarch of Aquileia, *In civitate Polæ habet duo antiqua Palatia, scilicet Jadrum et Harenam, et Palatium unum in platea civitatis et quasdam alias domos. Et quicunque accipit aliquem lapidem de dictis Palatiis Jadri et Harenæ, pro quolibet lapide quem accipit, solvit Domino Patriarchæ Byzantios centum*[c]. *Jadrum* is probably a corruption of *Theatrum*, as there was originally a theatre, as described by Serlio, on a hill without the town, which was destroyed by the Venetians, and its site occupied by a fort. In *harenam* we clearly trace the Latin word *arenam*. In a similar manner the ruins of amphitheatres in France are generally known by the term *Les Arènes*, whilst those at Bourdeaux are dignified by the title of *Le Palais Galiene*[d]. I merely mention this latter fact to shew that the writer of this manuscript was not singular in dignifying the amphitheatre at Pola with the title of *palace*.

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[a] The same idea prevailed with regard to the celebrated theatre at Taormina in Sicily, but recent excavations there have brought to light some stone seats.

[b] It was proposed by a Venetian engineer in the 16th century that the amphitheatre should be blown up: the inhabitants however of Pola had recourse to the Venetian senator Gabriel Emo, by whose intervention it was saved from such a melancholy fate.

The exterior of this building is rusticated, similarly to the amphitheatres of Verona and Nismes. As to the order of architecture to which it belongs, there has been much dispute. Palladio has pronounced the architecture both at Pola and Verona to be Tuscan, whilst Serlio and Bastiano of Bologna call it Doric. Nor is this a singular instance of the difference of opinion between professional judges on these subjects; for Nismes is said by Daviler to be Tuscan, and by others Doric, and the amphitheatre at Capua, which is called Doric by San Felice, and Tuscan by Mazocchio, is said by others to have combined the four orders which are found in the Coliseum. Unfortunately, there is no canon of Tuscan architecture remaining: whilst, according to Maffei, the Doric order is studied to the best advantage in the theatre of Marcellus[e], the Ionic in the temple of Fortuna Virilis, the Corinthian in the pantheon of Agrippa, and the Composite in the arch of Titus, the only specimen of Tuscan is found in the column of Trajan. Yet this, according to Forsyth, has a Tuscan base and capital, a Doric shaft, and a pedestal with Corinthian mouldings, whilst the column of Marcus Aurelius repeats the same mixture. The difficulty of distinguishing the Doric from the Tuscan order in Roman edifices arises in some degree from this circumstance, that the Roman Doric had its proportions quite different from the Grecian; for, as Forsyth observes, "in Roman temples, columns were a mere decoration, or at most supported the pediment above: in the Grecian, they formed an integral part of the edifice, not engaged in the wall, but rather the wall itself." Thus at Rome the ancient Doric column is about 7½ diameters in height; at Athens the greatest height is but 6; at Pæstum 4½; at Corinth only 4. The total disappearance of the Tuscan is to be attributed to its own rules. The Tuscan edifices, being aræostyle, had their intercolumniations so wide, that wood became necessary to form the architrave. Vitruvius[f], aware of the perishable nature of this material, gives particular directions as to the manner in which the beams should be laid: *Cum enim inter se tangunt, et non spiramentum et perflatum venti recipiunt, concalefaciuntur, et celeriter putrescunt.* I am inclined to acquiesce in the judgment of Palladio, and to pronounce the order of architecture at Pola, as well as at Nismes and Verona, to be Tuscan.

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[c] These were, probably, Byzantii albi (denarii), a coin which is frequently mentioned in the chartularies of monasteries. Vide Du Cange; article, *Byzantius*.

[d] There is a full account of this in vol. XII. Histoire de l'Académie des Inscriptions.

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[e] The lower tier of columns in this theatre is alluded to, as the upper is of the Ionic order.

The elevation consists of two orders of Tuscan pilasters, one above the other, the lower being placed upon pedestals on the side sloping towards the sea. The whole circumference is divided into seventy-two arches; the height into three stories, and by its particular construction, the uppermost story being an Attic with flat windows, displays an uncommon lightness and elegance of effect. There is a peculiarity about this building, which is not observed in any of the Italian or French amphitheatres. If we suppose the figure to be an oval, whose greater axis lies nearly north and south, at the intermediate distances in the outer circumference between the four cardinal points, there are four buildings, like towers, forming part of the circumference, which Serlio calls *contrafforti*, as if their purpose had merely been to strengthen and support the building. A more accurate examination of them has determined that they were staircases, so contrived that there might be two flights of stairs disposed in such a manner, that those who ascended by the one should not meet those who were descending by the other: there was room on each flight for two persons abreast. What was the precise object of these staircases is perhaps difficult to be determined. Some think that the Velarii ascended by them to arrange the awning, according as any sudden call for it should be made: others, that they merely served as *vomitoria* for the upper circles; whilst others again may be disposed to consider them as staircases which were reserved for the women to ascend by to their seats in the upper part of the building. Suetonius[g] mentions a decree of Augustus, which may serve to illustrate this latter conjecture, as females, who had hitherto mixed at large with the rest of the company, were prohibited by it from occupying any seats but those in the uppermost circles. The seats for the women, in accordance with this decree, may still be observed in the amphitheatre at Pompeii. These private staircases would be the more necessary, if females were altogether prohibited, in the same manner as at Rome by another decree of Augustus, from being present at the contests of the Athletæ, and were therefore often not allowed to occupy their seats at the commencement of the day's amusement.

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[f] Vitruvius, De Architect. lib. iv. c. 7. The intercolumniations were called *aræostyle* when the interval between the columns was greater than three diameters.

It had long been supposed that the interior of this amphitheatre had been entirely of wood, the exterior being so perfect, whilst not a vestige apparently remained of the interior. When, however, Istria was occupied by the French in 1810, some excavations were commenced in the arena by the directions of the duke of Ragusa, and as some important fragments were at once discovered, they were not discontinued on the subsequent cession of the country to the Austrians. The result was, that the plan of the arena and the podium was brought to light, and thus Maffei's absurd supposition, that it was a circular theatre, was completely overthrown. This author, being extremely puzzled to account for the four turrets, or staircases, assumes that two of them were on the stage side of his supposed theatre, and that the actors spoke from them in comedies, as from the windows of private houses, and that the other two were merely for the sake of preserving the uniformity of the building. This author may be dismissed at once with this observation, that the *Amor Patriæ*, condemned in Italy to circulate in such narrow spheres, had completely blinded his judgment, as his main object was to maintain the claim of his native town Verona to the possession of the only stone amphitheatre in the north of Italy.

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[g] Sueton. Aug. c. 44. Calpurnius Eclog. vii. 27.

Along the major axis of the arena there appears to have been a covered passage beneath, the roof of which was supported by pillars of the Composite order. It may either have been only for the purpose of affording a communication without crossing the arena, or have been connected with the dens of prisoners and wild beasts, as in the Coliseum and at Capua. A canal of about two feet in breadth, encircles the arena. This may have served merely for a drain, as there is a broad channel on the north-east which enters the arena from above, and communicates with this canal, and another on the south-west descending towards the sea; or it may have been a miniature Euripus, on the supposition that wild beasts were hunted here. The height of the podium[h] is only five feet, about the same with that of Verona. Stancovich argues from comparing this with the height of the podium of the Coliseum[i] (15 feet), that wild beasts were not hunted here, or at Verona. Unfortunately, however, for his hypothesis, there is still extant a letter from the younger Pliny[k] to Maximus, whom he is complimenting for having exhibited a gladiatorial show to the good people of Verona; where he says, *Vellem Africanæ, quas coemeras plurimas, ad præfinitum diem occurrissent, sed licet cessaverint illæ, tempestate detentæ, tu tamen meruisti ut acceptum tibi fieret, quod quo minus exhiberes, non per te stetit.* The animals, which are here called *Africanæ*, were panthers, which were highly prized for such exhibitions[l]. The elder Pliny[m] states, that these animals were first introduced to the Romans by Scaurus, in his ædileship, who exhibited 150 of them at once; and that there was a special law, allowing them to be imported into Italy for exhibition at the games. This allusion of Pliny to the fact, that it was in contemplation to exhibit these panthers at Verona, overthrows the argument of Stancovich with regard to the lowness of the podium being decisive evidence that wild beasts were not hunted either at Pola or

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[h] The podium is properly the projecting part of the wall that encircles the arena, but it is here used for the whole wall.

[i] According to Carli, that of Nismes was twelve feet high.

[k] Lib. vi. Ep. 24.

[l] "Pantheræ Africanæ, tantæ vivacitatis, ut exsectis visceribus, diu et acriter pugnent." See likewise the Ancyran Monument. Tac. Oberlin. iv. p. 845.

[m] Lib. viii. c. 16.

Verona. Perhaps a podium, five feet in height, surmounted with a strong balustrade, and an Euripus, two feet in breadth, so as to balk the animal in his spring, might be a sufficient safeguard to the spectators in the lower cavea against these comparatively small animals. At Rome, on the other hand, the podium was calculated to protect the spectators, not only against the ferocity and the courage of the tiger and the lion, but likewise against the immense bulk of the rhinoceros and the elephant.

The chief peculiarity of this amphitheatre is in the seats. Two or three fragments had already been discovered, when in the course of the excavations in 1810 a splendid seat was brought to light in the most perfect preservation, having certain lines, letters, and numbers marked upon it. Since that time not less than forty-two seats have been dug out of the ruins, some with letters upon them, others plain, and some with intermediate steps, where the ascent had been from one tier to another. The large seat above-mentioned has its upper surface divided into compartments by lines at the distance of 13 Venetian inches, or, as I ascertained by actual measurement[n], 14½ English[o]. This space of 14½ English inches, although not excessive, was found by us on experiment to be a fair allowance, and from the allusions in Latin authors we are led to suppose that the audience was packed tolerably close, as in the "cavea[p]" of modern theatres. To this Ovid[q] alludes in the following lines:

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[n] The author of this memoir visited Pola in the autumn of 1834.

[o] On this large stone five of these compartments may be observed: at one extremity there is a small space of five inches, and at the other one of nine, which probably was completed on the next seat. (Vide Plate.)

Quid frustra refugis? cogit nos *linea* jungi;
Hac in lege loci commoda circus habet.

The same space we found uniformly prevailed in all the seats. One of these seats is remarkable from a double row of lines. The first space marked off was only 11 Venetian inches: the mason did not perceive his error at first, and had drawn the two next lines at the proper distance of 13 inches, when he found himself obliged to commence again; and we have thus a series of pairs of lines two inches apart.

With regard to the breadth of the seats, by which is meant the space from the front to the back, the measure is not uniform: that however of the large seat above alluded to, and of eighteen others, is about 24 Venetian inches, equivalent to about 27½ English. The block of stone, however, did not terminate here, but was continued further backward, being lowered about half an inch, and thus forming a groove for the next succeeding upper stone to fit into. That the breadth of the seats in the Circus was not very great, we may also infer from Ovid[r]:

Tu quoque qui spectas post nos, tua contrahe crura,
Si pudor est, rigido ne preme terga genu.

The breadth of the remaining seats varies from 24 to 33 Venetian inches.

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[p] See Appendix.

[q] Ovid, Amor. l. iii. El. 2.: also Ars Am. l. i. v. 139.

[r] Am. iii. l. 23: also, Am. i. l. 159.

There is likewise some variation in the heights of the seats. But of the whole number of forty-three we have the large perfect one and eighteen others of the same height, $13\frac{1}{2}$ Venetian inches; the rest vary from $13\frac{1}{2}$ to 24 Venetian inches. It would appear from this that the minimum height is $13\frac{1}{2}$ inches, and if we allow half an inch for the stone to fit into the groove of the seat below, the minimum height of a seat was 13 Venetian inches, and the minimum breadth, as before mentioned, 24 Venetian inches. If, now, we refer to the directions given by Vitruvius [s], for the construction of the seats in theatres, we find the following words: *Gradus spectaculorum ubi subsellia componantur, ne minus alti sint palmopede, ne plus pede et digitis sex. Latitudines eorum ne plus pedes duo semis, ne minus pedes duo constituantur.*

The measure of the Roman foot has long been a *vexata quæstio.* If we adopt the calculations in the *Metrologie* of De Romé de l'Isle, there were three varieties of feet in principal use amongst the Romans. The Geometrical foot of 16 digits, the 600dth part of the nautical stadium [t], is supposed to have been chiefly employed by them in nautical and astronomical calculations. The Capitoline foot of $16\frac{3}{5}\frac{1}{2}$ digits, engraved by Luca Petto in the Capitol, and verified by Fabretti from a careful comparison of it with three ancient iron foot-measures dug up amidst some ruins of great antiquity, is what is more especially called the Roman foot, and is supposed to have been the foot in common use, by which the Roman roads were measured. The Philæterian, or royal foot, the 600dth part of the Philæterian stadium [u], is considered by De Romé de l'Isle to be the same as the palmipes, which consisted of 20 digits, being made up of the geometrical foot of 16 digits and the palm of 4 digits. The palmipes is considered by others to be made up of the Roman foot and the Roman palm, which would make its dimensions a trifling degree greater. On the former hypothesis, the palm is calculated as the sixth part of the common cubit. The following table will serve to facilitate the comparison of the different measures.

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[s] De Architect. v. c. 6.

[t] This stadium was the 400,000dth part of a degree, being the same as the Persian stadium, which Herodotus makes use of.

Names of measures.	Digits	Parts	Inches	Lines	100ths of a line
Palm	4	307	2	6	70
Geom. foot	16	1233	10	3	31
Capitoline, or Roman foot	16 [illegible]	1306	10	10	60
Philæterian foot, or palmipes	20	1542	12	10	18
English foot		1352	11	3	25
French foot		1440	12		
Venetian foot		1541	12	10	10
Veronese foot		1490	12	7	

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[u] The Philæterian stadium was the 318,000th part of a degree: it is supposed to have been introduced by Philetærus, the founder of the kingdom of Pergamus, B. C. 283.

The measurements are in French inches, &c., there being 12 lines to an inch. The proportion of the English to the French foot is now determined to be as 10000 to 10659, whereas that given by De Romé de l'Isle is as 10000 to 10649. The difference, however, is not of much consequence, as far as the calculations here are concerned.

The seats, then, according to Vitruvius, were not to be less in height than a palmipes, or 20 digits, nor greater than a foot and 6 digits (22 digits): in breadth not greater than 40 digits, nor less than 32. If we therefore adopt the calculation of De Romé de l'Isle as to the length of the palmipes, it corresponds almost exactly with the Venetian foot; and on this supposition the height of the seats falls within the prescribed limits of 20 and 22 digits, being nearest to the maximum, as 13 Venetian inches fall very little short of 22 digits. If, on the other hand, we assume the palmipes to be made up of the Roman foot and the Roman palm, its length will be 13 inches 6 lines 50 cents French; and as 13 Venetian inches are 13 inches 10 lines 94 cents French, the seats on this supposition approach the minimum. On this hypothesis, the maximum must be considered as made up of the Capitoline or Roman foot *plus* six digits; but I am rather inclined to adopt the calculation of De Romé de l'Isle, as on the other hypothesis the difference between the maximum and minimum will be a fractional portion of 2 digits[x], instead of an integral number of digits. In either case however the proportions of the seats harmonize with the directions of Vitruvius.

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[x] Or 1 inch 2 lines 65 cents French.

This circumstance is of the greater importance, as at Nismes, according to Maffei, and at Verona, the heights of the seats are about 26 digits. I would however observe, that, although the pride of the Veronese in their amphitheatre is founded on the whole range of its seats being entire, great numbers of them are comparatively modern, and none can be considered as coeval with the building itself. My own measurement of these seats made them out to be on the average 14½ English inches in height, the same as at Pola; but Maffei was a native of the place, and had many opportunities of verifying his calculations. Torelli, however, gives the height of them as 14 inches, probably in Veronese measure, and in that case equal to 14 inches 8 lines 50 cents French[y]. It may certainly have happened that Maffei, in his interpretation of the passage in Vitruvius, adopted the same idea as Stancovich, which I cannot but consider as erroneous, and imagined the foot, which is spoken of after the palmipes, to be the same as the palmipes, a foot of 20 digits, so that he made out the maximum height allowed by Vitruvius to be 26 digits, and took the greatest measurements that he could find in Verona. I observed in that amphitheatre that under many of the seats small pieces of brick, &c. were introduced. It would be a curious subject of investigation, whether the object of this was merely to supply what the ravages of time had wasted away, or to raise the seats somewhat higher at the time when the building was repaired, and thus to make them appear to confirm the interpretation of Maffei.

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[y] Torelli's measurements, I find, were in French feet: 14 inches French are but in a trifling degree greater than 13 inches Venetian.

Out of the whole number of forty-three seats, or fragments of seats, which have been found at Pola, nineteen are 13½ Venetian inches in height, and the remainder somewhat exceed this measurement in a greater or less degree. The most obvious way of accounting for these variations is this: that as one side of the amphitheatre was on the slope of a hill, the material of which is hard limestone, the irregular heights of these seats may have been adapted to different parts of the surface of the rock, where it was requisite to provide such a resource against accidental inequalities. The minimum height of the seats is by no means an inconvenient one, but they were probably raised somewhat higher by a layer of boards for the less privileged orders, and by cushions for the dignitaries of the place[z]. Not only, however, was the height of the seats not considered inconveniently low, but the reverse occasionally, as stools for the feet to rest upon are also alluded to by Ovid[a]. The object of these stools was no doubt partly to keep the knees of the spectators from interfering with the comfort of those who sat on the tier of seats below them.

With regard to the breadth of the seats, the maximum was, according to Vitruvius, 2 feet 8 digits, the minimum 2 feet. Accordingly at Pola the nineteen seats above-mentioned are exactly 2 Venetian feet in breadth, or 2 palmipedes, (40 digits,) the maximum allowance: beyond this the stone is still carried on, being lowered about half an inch, so as to allow the seat above to fit into it. The greater breadth of some of the other seats was perhaps requisite to give firmness to the building, by allowing the seat above to project further over that immediately below it, where the surface of the rock did not afford an adequate support to it. On two of the seats, which were evidently at the extremity of a *cuneus*, there are steps cut for the spectators to ascend by. These steps are seven inches French in height, about the same that Torelli has given for those at Verona, *nec alti plus quam uncias septem.* The breadth of the seats at Verona I ascertained to be 26 English inches, somewhat less than the maximum of 40 digits. This may be considered as a decisive confirmation of the opinion of those who agree with Carli, in fixing the minimum at 32 digits in opposition to Stancovich, who interprets the "pes" throughout the whole passage in Vitruvius as the "palmipes," and thus fixes the minimum at 40 digits, or 29 English inches.

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[z] Dion Cassius, l. lix. c. 7. Juvenal, iii. l. 153. Plautus Stichus, act. i. sc. 3.

[a] Et cava sub tenerum scamna dedisse pedem.

In the following table, the dimensions of the amphitheatre at Pola are given in juxtaposition with those of the amphitheatres at Verona, Nismes, and Rome.

	Pola.	Verona.	Nismes.	Rome.	
	Stancovich.	Maffei.	Menard.	Fontana.	
Major axis ext.	394	450	391	564	Veronese Feet.
Minor.	316	360	306	467	
Major ax. arena.	201	218.6		273	
Minor.	123	129		173	
Ext. circumference.	1127	1290	1101	1566	
Height.	89	110	63	140	
Number of arches.	72	72	60	80	
Tiers of seats.	35	43	35	67	

We may here observe, that the same proportion is maintained at Pola, Verona, and Nismes, between the exterior major and minor axis, which are all but accurately in the ratio of 5 to 4 to each other. (Vide Appendix.)

The discovery of the lines upon the seats, by which it appears that a space of 13 Venetian inches only was allowed as sitting room to each person, furnishes us with fair data for calculating the number of spectators that these amphitheatres would contain. Maffei, in his calculation of the contents of the theatre at Verona, allowed to each spectator a foot and a half of Veronese measure, nearly one third more than was allowed at Pola. On this hypothesis he estimated that the amphitheatre at Verona would hold 22,000 spectators. If his method of computing the number be assumed to be correct, it is evident that with our new data, the sum will come out much greater for the number of spectators, as 22,000 multiplied by 18 Veronese inches will equal 14½ English inches multiplied into the sum required. So that by a common[c] proportion we have

4901 : 6705 : : 22,000 : 30,097.

This is an important difference, if the number of spectators, that the amphitheatres could contain, is to be considered any criterion of the size of the towns, or of the population of the neighbourhood. During the congress at Verona, horseraces were exhibited in the arena of the amphitheatre; and when the course had been railed off, spectators were admitted to the seats, and to the centre part of the arena within the course. It was ascertained by the tickets received at the entrance that 60,000 persons were admitted on this occasion.

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[c] There are in 18 inches Veron. 2235 equal parts, of which there are 1633⅔ in 14½ English inches, so that the proportion is easily obtained from this.

At Nismes again, M. Menard made his calculation, allowing 16 inches French for each spectator; and on this hypothesis computed, that the amphitheatre there would contain 22,820 spectators, and that as probably only about a fourth of the inhabitants were present, the population of Nismes might be rated at 80,000. There is however an evident fallacy in this latter supposition, as no allowance is made for the inhabitants of the neighbouring country, who would of course flock in to the spectacle. We will however assume, that, excepting his data, his computation is made correctly, and we shall then have, in a similar manner as before, this proportion[d],

4901 : 5760 : : 22,820 : 26,799.

So that, from the new data that the discovery of these lines affords us, we may estimate the number of spectators at Nismes at about 4000 more than what M. Menard has assigned.

Stancovich calculates from the new data that Pola had accommodation for 26,000 persons; but as the amphitheatre there is somewhat greater in dimensions than that at Nismes, and its tiers of seats equal in number, his computation must evidently have been made on a more moderate scale than that of Menard. Menard having measured the greatest and least circumference of the seats, takes a mean proportional, and multiplies this by 35, the number of the tiers of seats, and thus obtains the number of persons by dividing this product by the space that he allotted to each person. Unfortunately, in the absence of these measurements at Pola, we cannot compare the numbers so accurately as might be wished; for with the only data[e] that Stancovich affords us, the numbers come out much less than 26,000. If, however, we look at the proportions of the axes here, as compared with those at Nismes, we may fairly consider that the amphitheatre would contain about 27,000 spectators.

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[d] There are in a French foot 1440 equal parts, of which there are in an English foot 1352.

The Coliseum at Rome is said to have held 85,000 spectators. This statement, although assailed by Maffei, who reduces it to 50,000, and by Giacomo Rucca, who brings it down to 34,000, the number which he assigns to the amphitheatre at Capua, is confirmed by the measurements of Lucangeli, whose celebrated model of the Coliseum is well known to most of those who have visited Rome.

I should state, that the three uppermost rows of seats at Pola are supposed to have been constructed of wood, a kind of gallery, such as is believed to have been generally fitted up in amphitheatres. Revett describes the upper part thus: "On the exterior, between the windows, are holes in the plinth, evidently for the supporters of the velum. The top is at present without mouldings, a singular finish, and in the interior there is a series of holes corresponding to those on the outside, with an additional one in the middle of the windows. It is not impossible that this amphitheatre had a portico of wood on the upper part, as directed by Vitruvius in theatres, but of which no vestige, I believe, remains in any existing ruin. A gutter extending along the top between each window, with triangular openings on the outside, as spouts to carry off the water, justifies the idea." From an inscription at Rome respecting the Fratres[f] Arvales, priests appointed to offer up sacrifices for the fertility of the soil, we learn that there was a similar wooden gallery in the Coliseum, as a certain number of places are enumerated as assigned to them in the first and second menianus, on the stone seats, (*gradibus marmoreis,*) and in the highest menianus on the wooden benches, (*in ligneis.*)

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[e] External circumference of the building 1090 Venetian feet; circumference of the arena 493 feet. Maffei, however, gives 1110 for the outer circumference, whilst his dimensions for the axes are less than those of Stancovich; so that the measurements cannot be sufficiently depended upon.

There is another peculiarity connected with the seats in this amphitheatre, that on several of them letters are engraved, as if to denote that they were private seats. It is well known, as we have just seen in the inscription respecting the Fratres Arvales, that certain seats were set apart for magistrates and corporate bodies; but it is not so clear that individuals were allowed a proprietary interest in seats. On the theatre at Syracuse there are some Greek characters engraved on the back of some seats, which lead to the supposition that they were assigned to distinguished personages, as three[g] inscriptions may be deciphered, but we have here the inscriptions in full, and not mere letters. Stancovich, however, has ingeniously endeavoured to find the owners for these seats at Pola, by identifying the initials on them with the names of persons, which occur in inscriptions at Pola or in the neighbourhood. His idea, that they were the private property of individuals, has some confirmation in M. Artaud's statement, that he found some inscriptions at Nismes, which he was led to suppose intimated places of honour for certain families; and on the amphitheatre at Pompeii we have inscriptions at full length, intimating that seats were assigned to individuals as marks of honour. How valuable the privilege of a private seat may have been, can be imagined from the fact, that places were not unfrequently occupied the night[h] before.

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[f] Marini. Gli atti dei Fratelli Arvali. Roma, 1795. p. 219, 224. Tav. XXIII.

[g] ΒΑΣΙΛΙΣΣΑΣ ΝΗΡΗΙΔΟΣ. ΒΑΣΙΛΙΣΣΑΣ ΦΙΛΙΣΤΙΔΟΣ. ΒΑΣΙΛΕΟΣ ΙΕΡΩΝΟΣ.

It now remains to determine the age of this amphitheatre. I am myself inclined to the opinion that the amphitheatres at Pola, Verona, and Nismes were all constructed about the same period. The style is the same in all, rusticated; the order of architecture, Tuscan. No cement or mortar is used in any of the three. The thickness of the walls at Verona and Pola, and, as I believe, at Nismes, diminishes from the inside, whereas in the Coliseum it diminishes from the outside. The latter plan Serlio considers to give more strength, the former more room. At Pola and Verona there are 72 arches, at Pola and Nismes there are 35 rows of seats. Pola, Verona, and Nismes were all Roman colonies.

The history of Pola is carried back by tradition to the Argonautic expedition, as its foundation is referred to a detachment of Colchians, who had been dispatched in pursuit of the conquerors of the golden fleece. When Istria surrendered to the Roman arms, U. C. 573, Pola of course lost its independence. We find it afterwards spoken of as a Roman colony by the elder Pliny, who, after enumerating the municipal towns of Istria, goes on to mention, *Colonia Pola, nunc Pietas Julia, olim a Colchis condita.* The medals dug up in the neighbourhood are all of the time of the first emperors. The beautiful temple of Rome and Augustus, the portico of which is one of the most exquisite specimens of Corinthian architecture, and the corresponding temple of Diana, evidently of contemporaneous origin, are evidence that the taste for architectural display, and the wealth requisite to indulge that taste, were found here as early as in the reign of Augustus. There are other ruins evidently both of an earlier and later date, but there is no token [i] about the amphitheatre which would serve to determine accurately the time of its erection. The question therefore resolves itself into one of probability.

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[h] Suetonius Caligula, c. 26. Inquietatus fremitu gratuita in circo loca de media nocte occupantium, omnes fustibus abegit. Elisi per eum tumultum viginti amplius equites Romani, totidem matronæ, super innumeram turbam cæteram.—The Circus Maximus is here alluded to, which lay between the Palatine and Avertine hills, and was overlooked from the palace of the emperor.

The earliest *stone* amphitheatre that we read of was built by [j]Statilius Taurus, in the reign of Augustus, from the encouragement of that prince; nor does this appear to have satisfied the wishes of Augustus, as he is said by [k]Suetonius, to have formed the design of an amphitheatre in the midst of the city, which Vespasian afterwards executed. Tiberius made use of the amphitheatre of Taurus, and Caligula likewise, but this latter prince commenced a new amphitheatre[l] near the Septa. This work, however, was discontinued by Claudius, who directed the wealth and labour of the nation to a more useful object, the Claudian aqueduct. Nero was satisfied with a wooden amphitheatre, which he built in a year's time, as that of Taurus was burnt down in his reign. His passion for architectural display found a vent in his golden house. The subsequent emperors, till the accession of Vespasian, seem to have had neither the inclination nor the leisure for such works.

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[i] I did not observe anywhere on this amphitheatre the bull's head, the symbol of a colony, which may be observed on those at Nismes and Verona. It might be argued from this, that the building was constructed prior to the time when Pola was made a Roman colony.

[j] Dion Cassius, LI. c. 23. This was U. C. 724.

[k] Suetonius, Vespasian, c. 9.

Maffei founds his argument against the early origin of these amphitheatres, on the improbability of the colonies having had stone amphitheatres before the building of that of Vespasian at Rome, which was dedicated by Titus in the last year of his short reign, and thus he would make them all posterior to the year 80 of the Christian era. Fortunately a specimen exists to which he had no access, and against which his doctrine is of no avail, the amphitheatre at Pompeii, the date of which is certainly anterior to 60, A. D. In the first place, this amphitheatre bears marks of having suffered by the earthquake of 63, A. D., as supports and bulwarks of various kinds have been added to it, and there is an inscription assigning places of honour to some individuals, who had contributed to the restoration of it: and in the second place, we are informed by Tacitus[m], that in the year 60, A. D., a murderous dispute arose at Pompeii, between the inhabitants of that town and the Nucerians, at a gladiatorial show exhibited by Levineius Regulus, in which the Nucerians had the worse, as the Pompeians were able to procure arms. In consequence, the Pompeians were prohibited by a decree of the Roman senate from holding such assemblies for ten years. The presence of the Nucerians at this show, is evidence that the inhabitants of the neighbouring towns formed part of the spectators, for whose accommodation the dimensions of these buildings were in general calculated. So far, then, the opinion of Maffei is without foundation.

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[l] Ib. Caligula, c. 21.

[m] Annal. l. xiv. c. 17.

We have, however, no actual data to enable us to determine accurately the epoch at which this amphitheatre at Pompeii was constructed. It would seem, however, to have been built before the smaller covered theatre or Odeum, as the names of C. Quinctius Valgus and M. Porcius, the duumvirs who built the latter, are inscribed on the amphitheatre as having exhibited some games to the colonists at their own expense, when duumvirs. There was also found a statue of C. Cuspius Pansa at the entrance of the amphitheatre, and an inscription recording a magistracy which he had discharged according to the Petronian law. The object of this law [n] was to prohibit masters from compelling their slaves to fight in the arena with wild beasts, unless previously condemned by the tribunals. It is not agreed in whose reign this law was passed. Heineccius and others refer it to C. Petronius Sabinus Turpilianus, who was consul in U. C. 814, during the reign of Nero; but Haubold and others with greater probability, I think, to an individual of the Petronian Gens, who lived in the reign of Augustus. In this case it must be referred to P. Petronius Turpilianus, and the law seems to have been commemorated by family coins, in the same way as the Mamilian and the Papian Poppæan. Of these we have two remarkable coins in the British Museum. One has on the obverse the head of Augustus, and on the reverse a man standing *cum patera et flagro*[o], with the legend *P. Petronius Turpilianus $\overline{III}$ vir;* the other has on the obverse the head of Augustus, and on the reverse the head of Feronia[p], the goddess to whom slaves offered up their vows in the hope of liberty, with the same legend round it, as on the other coin. That there may have been ample occasion for such a law in the reign of Augustus may fairly be presumed from Seneca's[q] account of the cruelty shewn on the most ordinary occasions to slaves; and its promulgation would be in accordance with the general character of that prince, and the spirit of his legislation. On the other hand, it is not very probable that an emperor like Nero[r], who could compel

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[n] Vide Noordkerk, disquisitio de lege Petronia, where he says, in allusion to the coins of the Petronian family, "Næ dici vix potest quî hæc cum Seneca conveniant. Ait Philosophus, positum fuisse qui et sævitiam et in præbendis ad victum necessariis dominorum avaritiam compescat." (Vide Seneca, de Benef. iii. c. 22.)

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[o] As described by Rasche in his Lexicon Rei Numismaticæ.

[p] The temple of Feronia was at Terracina, (Horat. I. Sat. v. l. 24.) and on a seat before her altar the following words were inscribed, "Benemeriti servi sedeant, surgant liberi." It was not unusual for fortunate slaves to dedicate altars to this goddess.

[q] Seneca de Ira. iii. c. 40. de Clement. i. c. 18.

[r] Sueton. Nero, c. xii. Juvenal. Sat. i. l. 156.

Roman senators and knights to fight with wild beasts in the arena, and could burn human beings at the stake, should have felt any romantic compunctions for the sufferings of slaves, whom their masters had pronounced worthless. It is on these grounds that I am inclined to agree with Haubold, and to suppose that this inscription [s] at Pompeii refers to the reign of Augustus, and to the officer on whom the duty of "Protector of Slaves" devolved according to the Petronian law.

The amphitheatre that we read of in Tacitus and Suetonius, the fall of which entailed such a dreadful loss of life in the reign of Tiberius, was certainly a temporary wooden one, like that of Nero's [t], in the Campus Martius. Tacitus, however, makes mention of a noble amphitheatre at Placentia [u], that was destroyed by fire in a conflict in the suburbs of that town between the troops of Otho and Vitellius, and states, that the common people there believed that it had been maliciously set on fire by some of the inhabitants of the neighbouring towns, from jealousy of its being the most capacious building of the kind in Italy. Maffei acknowledges the obvious inference from this, that there were at that time several large amphitheatres in Italy, but contends that this building must evidently have been constructed of wood, from its being destroyed by the fire. In reply to this it may be urged, that if it was merely a wooden building, it is improbable that its loss should have been so severely lamented by the rich town of Placentia, as Tacitus informs us was the case, since amphitheatres, of wood most probably, were constructed at Cremona and Bononia by the soldiers of Cæcina and Valens, within the short space of forty days, after the battle of Bebriacum, in order that the triumphant entry of Vitellius into Italy might be welcomed with shows of gladiators, his favourite amusement. In the second place, it by no means follows, from the circumstance of the conflagration, that it was a wooden building. For in the reign of Macrinus, A. D. 218, the Coliseum was struck by lightning; and Dion Cassius[x], who witnessed the conflagration, informs us, "that it was burnt from top to bottom; that all the upper gallery, which perhaps comprised a construction of woodwork raised on the top of all we now see, was consumed, and all the seats likewise; and notwithstanding the quantity of water which was brought to extinguish the fire, added to the abundance of rain which fell in the thunder-storm, the building was so effectually destroyed, that for several years it could not be used for exhibitions." The succeeding emperors contributed to its restoration; but we hear of no more shows exhibited there until the emperor Philip celebrated the secular games, A.D. 248[y]. I do not therefore conceive that the words of Tacitus at all prohibit the conclusion that this amphitheatre at Placentia was a stone-building, but rather the reverse, and that there may have been several other stone amphitheatres in the north of Italy about the same period.

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[s] C. CUSPIUS. C. F. PANSA. PATER. D. V. I. D.
IIII. QUINQ. PRAEF. ID. EX. D. D. LEGE. PETRONIA,
which may be interpreted, "duumvir jure dicendo quatuorvir quinquennalis præfectus idem ex decreto decurionum lege Petronia."

[t] Ann. iv. 62. Sueton. Tib. xl.

[u] Hist. ii. c. 21.

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[x] Liber lxxviii. c. 25.

[y] Vide Burgess, Antiq. of Rome, Dissert. v.

If, however, any argument may be founded on the similarity of the amphitheatres at Pola, Nismes, and Verona, both in the style and order of their architecture, which seems by no means unallowable, as such a similarity both in their proportions and workmanship may be observed in the contemporaneous amphitheatres of a later period[z], we shall perhaps be nearer the solution of our original question, if we can determine the period at which the amphitheatre at Nismes was erected.

We have then at Nismes the temple known by the modern appellation of *La Maison Carrée.* This was for some time supposed to be the Basilica of Plotina, built by Adrian; but it is now decided to have been constructed in the reign of Augustus, and to have been dedicated to Caius and Lucius Cæsares, the adopted sons of Augustus, who were honoured with the title of *Principes Juventutis*[a]. Again, the aqueduct, not far distant from Nismes, known as the *Pont du Gard*, is conjectured to have been commenced by Agrippa, in his office of *Curator Aquarum*, in the year U.C. 735, when he was resident in Gaul, and to have been completed in U. C. 750. The colony at Nismes had been established as early as U.C. 727, when Augustus was marching through Gaul against the Asturians and Cantabrians, and was composed of veterans from the army of[b] Egypt. Agrippa likewise superintended its organization.

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[z] Such as those built in the provinces by the Antonines.

[a] A similar temple was erected in honour of them at Rome. Sueton. Aug. xxix.

Assuming, then, that the supposition is correct as to the *Maison Carrée* having been dedicated to Caius and Lucius Cæsares, we perceive by the inscription on the portico, according to the deciphering of it by M. de Seguier[c],

C. CÆSARI AUGUSTI F. COS. L. CÆSARI . AUGUSTI . F.
COS. DESIGNATO PRINCIPIBUS JUVENTUTIS,

that Caius is spoken of as consul, and Lucius as consul elect. This then must have been between the years 754–757, as Caius, having been declared consul elect in 749, entered upon office as consul in 754, and Lucius had been declared consul elect in 752. They were both declared consuls elect in their fifteenth years, as we may learn from the Ancyran[d] monument, and remained for five years in that state of probation, during which however they were privileged to be present at the public councils.

But there are some other circumstances, which serve to connect the epoch of this temple with that of the amphitheatre; and amongst others, the following inscription, found in 1810 near the amphitheatre:

C. C. AVGVSTI. F. PATRONVS . COL
XYSTVM . DAT.

If the word *xystus* may be here interpreted according to the sense which Vitruvius attaches to it,

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[b] The medals of Nismes bear on the obverse the heads of Augustus and Agrippa averse and the letters P. P.; on the reverse a crocodile chained to a palm tree, and the letters COL. NEM.

[c] This inscription was ingeniously deciphered by fitting metal letters to the holes and marks on the frieze of the architrave.

[d] Tacitus, Oberlin, vol. iv. p. 841.

l. v. c. 9, as a covered portico, in which the athletæ were exercised in winter, it would not be unfair to conclude that these exercises were carried on with reference to exhibitions in some public place, and hence that there was an amphitheatre for such exhibitions built at a period preceding, or at least contemporaneous with the establishment of the training ground.

Some additional light may be thrown upon this subject by a fragment of an inscription found in the arena in 1809, with the words VIII. TRI. PO. as if alluding to the eighth year of the enjoyment of the tribunician authority by one of the emperors. Those who deny the existence of amphitheatres *at all* in the provinces till after the reign of Tiberius, and of *stone* amphitheatres there till the reign of Vespasian, conclude from this inscription, which they suppose to refer to the emperor in whose reign the amphitheatre was built, that the amphitheatre must have been built in the reign of Vespasian, Titus, or Domitian, as none of the emperors between Tiberius and Vespasian enjoyed the tribunician authority eight times. But if the first hypothesis is unfounded as to the provinces not possessing amphitheatres till after the reign of Tiberius—for it is only an assumption, and may be as baseless as the second hypothesis, which has been proved to be false by the evidence of the amphitheatre at Pompeii—we may find a more obvious explanation of these words.

On a gate at Nismes, known as the gate [e] of Augustus, there is the following inscription:

IMP. CAESAR . DIVI. F. AVGVSTVS. COS. XI. TRIBV.
POTEST. VIII. PORTAS. MVROS. COL. DAT.

This gate seems to have been erected by Augustus, when he had enjoyed the consular office for the eleventh time, and when invested with the tribunician power for the eighth. It is not an overstrained hypothesis to refer the inscription found in the arena to the same epoch. The eighth year of Augustus's tribunician authority began June 27, U. C. 738, and expired June 26, U. C. 739. He had discharged the office of consul for the eleventh time in 731, and did not again assume it till 749; so that we can here accurately determine the period to which the inscription on the gate refers. But in this very year 738, Augustus in person visited Gaul[f], having appointed Statilius Taurus viceroy of Italy, and having dispatched Agrippa on his second expedition into the East. Agrippa had himself been in Gaul in 734, on his way into Spain to quell an insurrection of the Cantabri, which he accomplished either in that or the following year. How long he remained in Gaul is uncertain, but it may have been on his victorious return from this expedition that the foundations of the amphitheatre were laid. It may thus have been completed at the time when Augustus visited Nismes, who may have presided at the dedication of it, or at all events was present on the occasion which the inscription appears to commemorate.

With regard to Verona, a military colony had been settled there as early as U. C. 666; and the importance of this town may be inferred, from its being selected as the pivot, upon which the armies

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[e] Amongst the other ornaments of this gate are two bulls' heads, the symbol of a colony, which I merely allude to as being likewise to be observed over the northern gate of the amphitheatre.

[f] Dion Cassius, lib. LIV. c. 19.

of Vespasian were to move in the expedition against Vitellius. Pliny, in his letter to Maximus, does not allude to the amphitheatre at Verona as either a new or singular ornament to the town; and we know from the various inscriptions that have been discovered, that this town was both able and inclined to indulge in architectural display in the reign of Augustus. The argument here, however, is not of a *positive* character, as far as records are concerned, but must chiefly rest upon the identity of the style of the building with that at Nismes.

At Pola likewise we have no *positive* evidence, excepting so far as to prove that the taste for architectural magnificence had diffused itself here in the reign of Augustus. Were I called upon to assign any period in the reign of Augustus as that in which it is most probable that the amphitheatre here was constructed, I should be inclined, from the staircases to which I have alluded as a peculiar feature in this building, to fix upon a period not long after the decree of Augustus which Suetonius records, by which females were no longer allowed to sit in the body of the house, but were confined to the upper circles. This regulation was perhaps one amongst others in the *Lex Julia Theatralis*. The year, in which this law was passed, is not precisely known; but it was most probably either the year 737, U. C., when the secular games were celebrated, and when by the *lex Julia de maritandis ordinibus*[g] seats of honour were assigned to the married men, or the year 741, U. C., when the pontifical games were celebrated by Augustus on his assuming the title of *Pontifex Maximus*. On this occasion he forbade females to be present at the shows of the athletæ, and may have limited their privileges in other respects. I should, however, rather incline to the earlier date, from the manner in which Suetonius relates the two circumstances. If this hypothesis be correct, the amphitheatre at Pola must have been commenced not long after that at Nismes was completed. We may fix upon the same period as that about which the amphitheatre at Pompeii was constructed. The *lex Petronia* which is recorded there, was passed, according to Haubold, in U.C. 763; and it is not improbable that the statue of Pansa was erected there soon after the law was passed, perhaps in commemoration of the magistrate on whom the pleasing office first devolved of mitigating the horrors of the arena.

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[g] Then first promulgated, but not passed into a law till 757.

The argument, then, which I have endeavoured to substantiate, may be summed up in these words. That it is by no means improbable that permanent stone amphitheatres were built in the colonies and municipal towns in the reign of Augustus: that the law of Petronius was passed in that reign, and would be more particularly called for on this supposition: that the amphitheatres at Pola, Verona, and Nismes, from their similarity, I may say identity, of style, are of contemporaneous origin; that their style is evidently anterior to that of the Coliseum built in the reign of Vespasian, or of the amphitheatre at Capua, which was built by that colony in imitation of the Coliseum, and dedicated by Antoninus Pius: that there is evidence almost of a *positive* nature that the amphitheatre at Nismes was built in the reign of Augustus: that the amphitheatre at Verona is not spoken of by Pliny as by any means new or strange in his time, and that the town was sufficiently wealthy to raise such a building, and also did evince a taste for architectural display in the same reign: that at Pola the temple of Augustus, and other buildings, lead to a similar inference: and that the encouragement given by Augustus to the building of such edifices was greater than that shewn by any other prince.

We have likewise positive evidence that Agrippa built many of the most splendid edifices in Rome; and that this was apparently so far a family taste, that his sister Pola built a portico and embellished the race course. That he was particularly interested in Nismes may be inferred from the works which he executed there, and from his son Caius being patron of the colony. I will not venture to assert that Statilius Taurus, when appointed prefect of Italy in 738, U. C., encouraged the building of amphitheatres; or that Pola, the sister of Agrippa, was likely to have contributed to the embellishment of a town which bore her name; but this, I think, may be maintained, that there is no evidence of so conclusive a nature as to warrant us in referring these buildings to the age of Trajan or of the Antonines, but that the probability is in favour of a much earlier date, both from the known encouragement given to such works by Augustus, the architectural remains of buildings decidedly raised in these towns during his reign, and the style of the amphitheatres themselves, clearly of an age less fond of ornament, and less corrupt in its taste, than that even of Vespasian and Titus.

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APPENDIX.

On the oval form of the arena in Amphitheatres.

THE form of all amphitheatres is an oval, more or less approaching to an ellipse, and in some cases said to be perfectly elliptical. The reason why this figure should be preferred to the circle admits of two explanations. The first and most obvious is, that it was more commodious for the spectators: "parcequ'elle était la plus avantageuse et la plus propre à faciliter et rendre générale à tous les spectateurs la vue des exercises." Such is the opinion of Mr. Ménard, which is partially confirmed by Cassiodorus[a]. The second reason is, that in an arena of this form the combatants would be able to retreat from, and advance upon each other through a greater space, and thus afford more excitement and amusement to the spectators. I might likewise add a third reason, that the majority of the spectators would be more comfortably arranged than in the spoke-like fashion of the radii of a circle. These arguments, however, only tend to shew that the circle was not the best figure, and not to explain why an oval of fixed dimensions should generally have been adopted. On this subject I will venture to suggest a few hints, not as decisive of the question, but as appearing to throw some light upon it.

The amphitheatre was merely two theatres without their stages, united at the stage extremities. The first that we read of was that of Curio[b], who, unable to rival Æmilius Scaurus in the splendour of his theatre, devised the novel scheme of two large theatres of wood adjoining each other, and suspended on hinges; in both of which he acted stage-plays in the morning, and in the evening, having wheeled them round, formed thus an amphitheatre, in which he exhibited shows of gladiators. This likewise is confirmed by Cassiodorus[c]. The earliest fixed amphitheatre, however, was built by Julius Cæsar of wood, and is spoken of by Dion Cassius[d] as a hunting theatre, and, according to him, received the name of amphitheatre, from there being seats all round without any stage: we may thus then consider the amphitheatre as made up of two theatres. There were,

[a] Cassiodorus, Variar. l. v. Ep. 42. Ovi specie ejus arenam concludens, ut concurrentibus aptum daretur spatium et spectantes omnia facilius viderent, dum quædam prolixa rotunditas universa collegerat.

[b] Pliny xxxviii. c. 15.

[c] Cassiodorus, ibid. Cum theatrum, quod est hemisphærium, Græce dicatur, amphitheatrum quasi in unum juncta duo visoria recte constat esse nominatum.

[d] Dion Cassius, 43. c. 22.

however, two kinds of theatres, the Roman and Grecian, the proportions of which were different, as in the former the object was to have as wide a stage as possible, since all the actors appeared on the stage; in the latter, since the chorus danced in the arena, it was desirable to have as deep an orchestra as possible. The proportions are given by Vitruvius, both of the Roman and Grecian theatre [e]. In the former the stage was marked off by the base of an equilateral triangle described in the arena, in the latter by one of the sides of a square. This enables us easily to calculate the depth of the orchestra in both cases. As the dimensions of the *arena* in the amphitheatre would doubtless be mainly looked to, we should, *à priori*, conclude that the Greek rather than the Roman theatre would be the element of the amphitheatre, as the dimensions of the arena in the former are greater [f]. If this conclusion be correct, the proportion of the greater to the less axis in the amphitheatre would be as 3.4142136 to 2, or nearly as $3\frac{4}{10}$ to 2. On examining the major and minor axes of the arena at Verona, the proportion appears to be as 218.6 to 129, which is strikingly accurate, for 218 is to 129 as $3\frac{4}{10}$ to 2. At Pola the proportion of 201 to 123 is not quite so accurate, since $196\frac{8}{10}$ is to 123 as $3\frac{4}{10}$ to 2. It is, however, so close an approximation as to be strongly confirmatory of the hypothesis. I have unfortunately no data in the case of Nismes. The proportions of the arena of the Coliseum are not the same, but this is what we should not be surprised at, if the age of the other three amphitheatres has been rightly determined.

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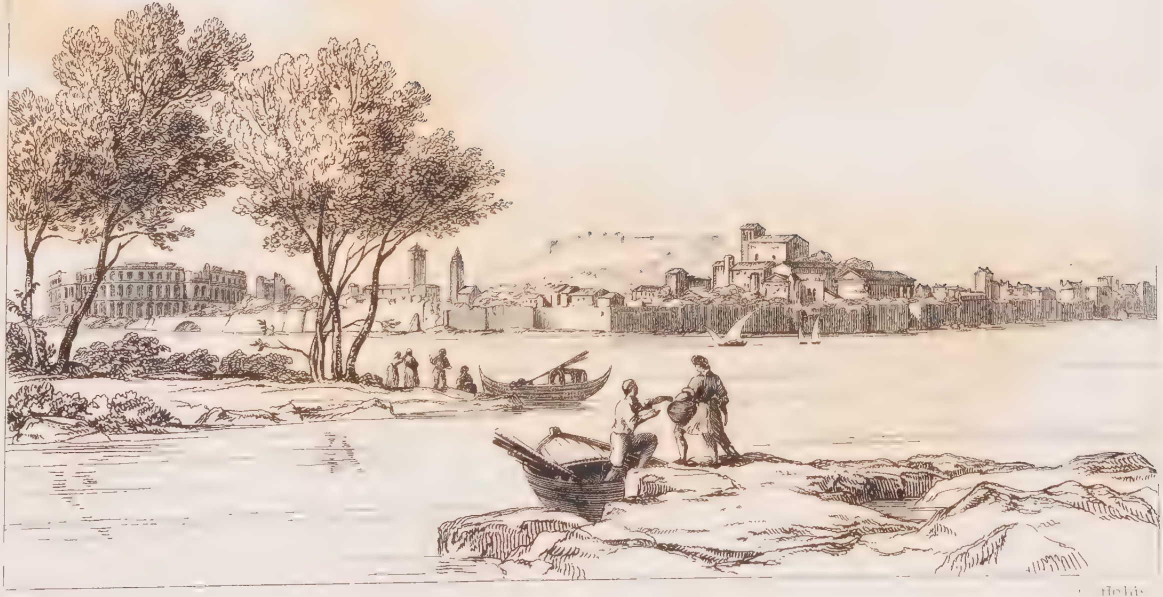
[e] Vitruvius, lib. v. c. vi. c. viii.

[f] In the Roman theatre if a diameter be drawn across the arena, parallel to the stage, the distance from that diameter to the stage = sine of 30° = $\frac{1}{2}$, or .5000000. In the Greek it = sine of 45° = $\sqrt{\frac{1}{2}}$ = .7071068, so that on the supposition that the amphitheatre was made up of two Greek theatres, the major axis of the arena would exceed the minor by twice this quantity, or would be to it in the proportion of 3.4142136 to 2.



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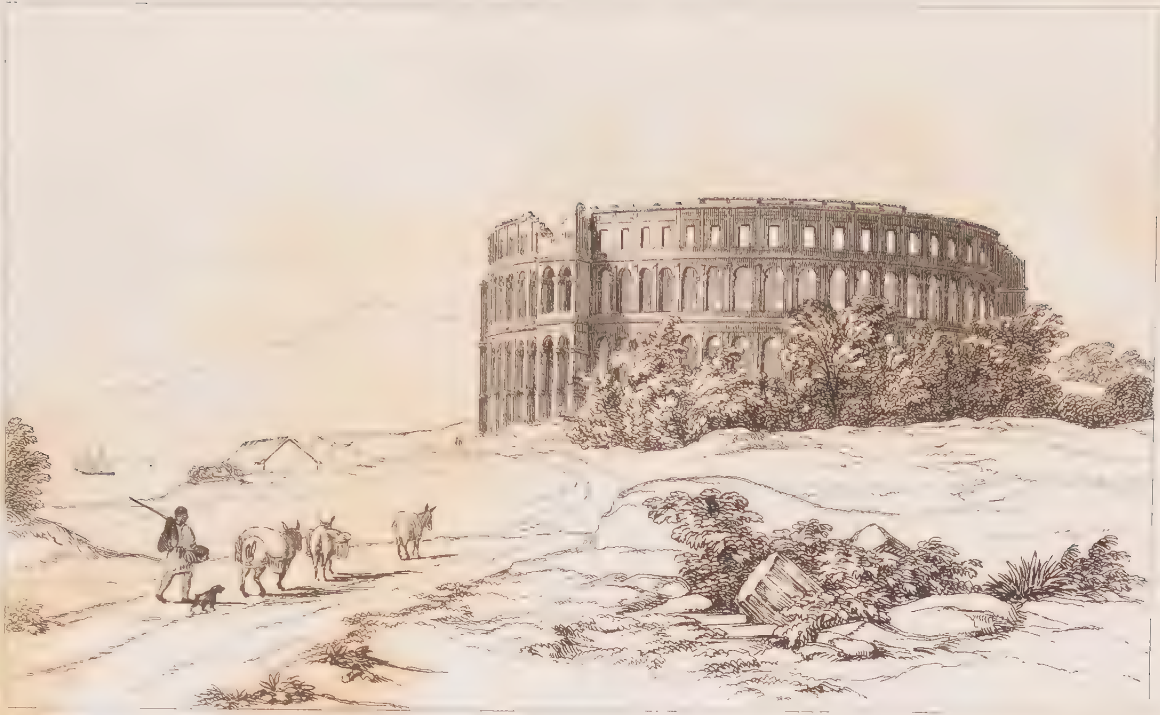

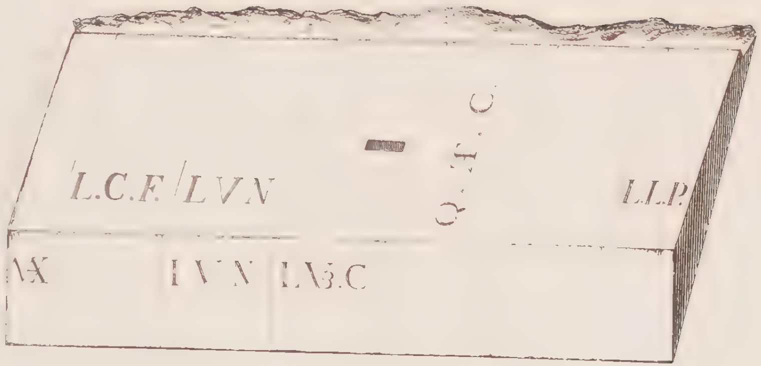
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LAB.C

ON

THE ARENARIUS

OF

ARCHIMEDES.

BY

STEPHEN PETER RIGAUD, M.A.

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OXFORD,
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ON

THE ARENARIUS OF ARCHIMEDES.

READ TO THE ASHMOLEAN SOCIETY, NOV. 11, 1836.

OF all the mathematicians of antiquity, Archimedes is confessedly the first for power and originality. Every one knows his use of hydrostatics in the examination of Hiero's crown, and the extraordinary machinery by which he enabled his countrymen to defend the city of Syracuse against the Romans: his geometrical works are efforts of transcendent skill; and in his treatise, entitled *Ψαμμίτης*, or Arenarius, he shews the means of accomplishing what in his time appeared to all others to be impossible. This treatise stands, from its subject, distinct from the rest of his works, but it is not on that account the less interesting or valuable. It gives us, indeed, no specimen of that beautiful geometry, in which the ancients taught by their example the most perfect form of close and logical reasoning; but if its want of this attraction has made us less familiar with it, the same circumstance adds a certain variety to the method of investigation which it pursues.

Archimedes was related to the kings of Sicily, and addresses his work to Gelo the eldest son of Hiero. It appears that the grains of sand at Syracuse had been held by some to be infinite, and that even those who could not admit such an unqualified assertion, still considered them to exceed any numbers that could be assigned for them. This seems to be a speculation from which no practical advantage was likely to be produced, and none possibly was derived from the mere resolution of the question; but in the means which Archimedes devised for this purpose, we find the principle not only invented, but brought into actual operation, which in our later times has formed one of the greatest means of shortening labour in the conduct of arithmetical processes. The excellence of the Oriental numerals has reduced the Grecian arithmetic to an object of historical curiosity, and we can only admire the ingenuity of those who could work with such awkward implements. The Arenarius, indeed, is employed rather on the arrangement than the notation of numbers, but the imperfection of that notation would, in any common hands, have probably soon put a stop to the inquiry. No one can read the treatise without finding how much more clearly he can follow the reasoning of it, by reducing the several parts to the figures which we have now in use; how much, therefore, must the difficulty have been increased when the ideas to be expressed were entirely new! Archimedes, however, confident of his powers to overcome the difficulty, at once endeavoured to take it in its greatest possible extent, and asserted that he could assign the numbers which should exceed not only the sands of Syracuse and Sicily, but of what would be sufficient to fill a sphere, equal to the earth, or even to the world itself.

Κόσμος, by most of the ancient astronomers, was understood to denote a sphere, of which the radius was equal to the distance of the sun from the earth; but Archimedes mentions that the views of Aristarchus had given it a wider sense, and he is thus incidentally led to describe one of the most precise accounts which has been preserved to us from antiquity, of the true system of the universe: ὑποτίθεται τὰ μὲν ἀπλανῆ τῶν ἄστρων, καὶ τὸν ἅλιον, μένειν ἀκίνητα· τὰν δὲ γᾶν περιφέρεσθαι περὶ τὸν ἅλιον, κατὰ κύκλου περιφέρειαν, ὃς ἐστὶν ἐν μέσῳ τῷ δρόμῳ κείμενος. It is then added that the fixed stars are set in a sphere concentric with the sun, and that the radius of it has the same ratio to the circle in which the earth revolves, as the surface of a sphere has to its centre. This latter part of the statement alarmed the rigid geometry of Archimedes, who points out that the proportion is thus made to belong to quantities, between which there is no analogy. This is so true, that a definite meaning can hardly be extracted, with any confidence, from the expression; and Archimedes has justly objected, that the centre, being a point, has no magnitude, and consequently can have no ratio to a surface; it may, indeed, be added, that this surface itself can have no definite magnitude independent of the radius with which it is supposed to be generated: still it must be borne in mind that we have not the original treatise of Aristarchus, the context of which might assist us in understanding the manner in which he may have intended to use this expression. It is not impossible that he may have thought the distance of the fixed stars so great, that the very circle of the earth's orbit was no more than as a central point, with regard to the spherical superficies in which they were placed. This sublime idea may appear to partake too much of modern discoveries, but it is clear that Aristarchus had advanced far beyond the views of his contemporaries; and although he still retained the common impression of the fixed stars being all at an equal distance from us, this theory is even now abandoned only from reasoning on general analogies, not from demonstration. But after all, there was nothing here for the geometer to stand upon, and such an immeasurable magnitude would indeed have made it impossible to have met the problem in the extent under which Archimedes was willing to take it; but he might have limited his conditions, and could have easily done so, without diminishing their general effect.

Archimedes, however, endeavours to extract a manageable meaning from the description of the system of Aristarchus. Unwilling to abandon the idea of the sun's motion, he says, that as the earth was generally taken for the centre of the universe, the intended ratio must have been, that its magnitude was the first of three proportionals, of which the second was the κόσμος, (in the common acceptation of the word,) and the third the sphere of the fixed stars. This gave him quantities which must have been considered as an ample allowance for the size of his largest space: indeed in all his calculations he uniformly endeavours to exclude all cavil and objections, by going beyond what might be necessary for establishing the truth, and in consequence is led from any attempt at minute accuracy in his fundamental quantities. For his smallest body he concludes that a sphere, one δάκτυλος in diameter, could not be supposed to contain more than 640 millions of grains of sand; and for his largest receptacle, that the sphere of the fixed stars might be taken with a diameter of not more than 100,000,000,000,000, or 100 million of millions of stadia. According to Arbuthnot, the *στάδιον* contained 9600 *δάκτυλοι*, but Archimedes takes it in round numbers, as not exceeding 10,000: and the contents of spheres varying as the cubes of their diameters, a general notion may be formed of the enormous extent to which the larger number must be increased, in order to specify how many grains of sand would be required to fill such a universe as is here imagined.

With all the advantages of modern arithmetic, our common enumeration would soon become burdensome in such a problem; but the difficulty of it was increased in an inconceivable degree, by the Grecian system of marking distinct numbers by the successive letters of the alphabet. With the addition of three symbols (ϛ ϟ ϡ) for 6, 90, and 900, they were thus able to denote all the digits, tens, and hundreds; a peculiar mark, not unlike an *iota subscriptum*, converted the nine first symbols into designations for so many thousands, and M stood for a myriad, the multiples of which were denoted by numeral letters which were annexed to it. Mere addition in such an arrangement required a strain of attention, but the trouble and tediousness of the work were still greater in multiplication. This labour was even further increased by their practice of working from left to right, beginning with the highest number in the multiplier, and the highest in their quantity which was to be multiplied. By these means, when multiplying four figures (for example) by four, they produced sixteen different results, which were afterwards to be amalgamated in order to determine the product of the whole. Accustomed as we are from our childhood to the use of the Oriental ciphers, the carrying on the tens seems to be very simple,—and so it is in practice; but the original introduction of this most useful process is wholly artificial, and was of itself by no means originally obvious. The first attempts in endeavouring to grapple with the difficulty, were clearly to secure the largest products, and to correct them by the addition of those which were of smaller value. This has in consequence been found to be the method adopted by self-taught reckoners. In 1827, a lad, of the name of Noakes, was exhibited here in Oxford, who possessed great powers of working sums without paper; and circumstances, which escaped him, led to the supposition of his multiplying from left to right. Upon inquiry this was found to be the case; and such is the force of habit in these operations, that when the contrary order was pointed out to him, he did not seem to appreciate the advantage of it. Another distant and unconnected instance may be brought forward in confirmation of the same view. In January 1712, an account was sent from Dublin to the Royal Society of a boy of 7 or 8 years of age, who worked large sums in his head in this same way; and Dr. Halley remarked at the time, that it was the order in which the Greeks proceeded in their multiplication; their numerals, indeed, were not calculated to suggest the advantages of the contrary method.

The *myriad* was the highest quantity for which there was a specific name in the common language of Greece, and this they went on repeating for the number of times which the occasion might require.

Archimedes saw that by these means the myriad itself became consolidated into a unit, of which 100, 1000, or any multiple might be taken, until the system, as it were, limited itself by reaching a myriad of myriads. He conceived the idea therefore of going on by analogy, and, calling the first his *πρῶτοι ἀριθμοὶ*, to consider the *μύριαι μυριάδες* of them as the monad of his *δευτέροι ἀριθμοὶ*, of which 10, 100, 1000, might again be taken, until they reached in amount to the myriad of myriads which would form the monad of *τρίτοι ἀριθμοί*. In this manner, the myriad of myriads of each order became the monad of that which immediately followed it, until this enumeration arrived at the *μυριάκις μυριοστοὶ ἀριθμοί*: of these the myriad of myriads might be taken, and here there would be a limit: it would, indeed, require an unit with 800 millions of our ciphers to express the value of it, but still it would be a termination, to provide against which Archimedes denominates all this vast accumulation the *πρῶτα περιόδος* of his system, and consolidates the highest of them into a monad of the *πρῶτοι ἀριθμοὶ* of a *δευτέρα περίοδος*, in which he goes on, till having reached the myriad of myriads of the *μυριάκις μυριοστοὶ ἀριθμοὶ*, the last of them becomes the monad of the *πρῶτοι ἀριθμοὶ τᾶς τρίτας περιόδου*. Thus, he says, we may go on to the completion of the *μυριάκις μυριοστὰ περίοδος*. He proceeds no further in the detail; but it is evident that the last number in this enormous period might, in the same way, be taken for the monad of a new series, and so the system might be continued to any extent.

In modern numeration we have what in a certain degree resembles this plan; for a million of millions makes a billion, a million of billions a trillion, and so on; but these words have not been determined for any very high multiples, and even the billion, the first of them, is hardly ever used, while the rest are so seldom met with, that they hardly belong to common language; and instead of constituting a regularly developed system, they seem rather to be mere examples of the manner in which such names might, if required, be constructed. Now a comparison of the suggestions of Archimedes with what later times have thus adopted, enables us to see the superiority of his mind. The monads in both cases go on in geometrical progression, in which our common ratio would be a million, but his myriad of myriads is 100 millions; so far he has the advantage; but this, though not trifling, is nothing compared with his contrivance for continuing the numeration. In cases of this kind, the attainment of the end depends in great measure on arrangement, and the application of suitable notation and distinctions. Archimedes himself, for the quadrature of the spiral, introduces preliminary propositions, which are founded on the principles by which figures are supposed to be generated in the method of fluxions. Maclaurin[a] has actually made use of them in his valuable treatise on that branch of science, with a particular acknowledgment of the author from whom they were taken; but Archimedes did not generalize the ideas, and adapt expressions to them, by which they might be conveniently brought into extended application. The consequence was, that this most prolific germ re-

[a] Page 59. art. 16.

mained for centuries without its due expansion, and his method of reasoning, which under more favourable circumstances has been the basis of the modern analysis, remained only the noble instance of his mighty individual powers. For the numeration, however, of the high numbers to be used in the Arenarius, he completed the whole system in the most perfect manner; and by assigning general descriptive names to the several parts, he avoided on one side the multitude of arbitrary specific denominations, which would soon have grown overwhelming to the memory; and on the other side he avoided the use of mere symbols, which it would have been impossible to have supplied in sufficient copiousness, after the method of the Greek notation, or to compress into any reasonable space, even now when we have recourse to the Oriental ciphers.

Simplicity and perspicuity were not the only advantages obtained by Archimedes. These large numbers were to be multiplied together, and as they increased, the difficulty of this process might naturally be expected to increase with them; but his arrangement enabled him to point out a method, by which this might be done with as little trouble for the highest orders of his periods as for his lowest numbers. He observed, that beginning from 1, and taking his monads in uninterrupted succession, the multiplying any two of them together would produce a monad, which would lie as far beyond the larger factor, as the smaller was removed from unity[b]. From modern algebra we learn, that when two powers of a quantity are to be multiplied together, the sum of their exponents is equal to that of their product; but this is not all. Archimedes took his geometrical series so as to begin from unity, and if MM be used to denote the myriad of myriads, it will become both the second term and the common ratio; thus MM × MM is the monad of the third order of numbers, MM × MM × MM is the monad of the fourth order, and so on. This series will therefore really be $(MM)^0$, $(MM)^1$, $(MM)^2$, $(MM)^3$, $(MM)^4$, &c. Hence the exponents will go on in arithmetical progression from 0, and at the same time answer respectively to the several terms of a geometrical progression beginning with unity; they thus become so many logarithms, of which the fundamental number, answering to 10 in Briggs's system, is taken as a myriad of myriads. These exponents, however, are in each case one less than the designation of the order of numbers to which the several monads belong; $(MM)^1$ is the monad of the *δευτέροι ἀριθμοὶ*, $(MM)^4$ the monad of the *πεμπτοί*: hence in using them Archimedes refers to the distance of his terms from unity, and not to the order of numbers to which they belong. As his only object was to shew the possibility of enumerating a large quantity, no interpolation was necessary for numbers between the complete powers; and indeed the arithmetic of the time would have made any attempt of that kind unavailing: but still, the principle on which he proceeded is virtually the same which, after the lapse

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[b] The manner in which he arrives at this conclusion is very curious: *ἔστων ἀριθμοὶ τινὲς ἀνάλογον ἀπὸ μονάδος, οἱ α̂, β̄, γ, δ, ε̄, ζ̄, η, θ, ι, κ, λ, μονὰς δὲ ἔστω ὁ ᾱ· καὶ παραπολλαπλασιάσθω*

ὁ $\overline{\delta}$ τῷ $\overline{\theta}$· ὁ δὲ γενόμενος ἔστω ὁ $\overline{\chi}$—precisely as it would at present be assumed that the quantity to be found was x. He then proceeds regularly in his demonstration, until he shews that ἴσος ἐστὶν ὁ $\overline{\lambda}$ τῷ $\overline{\chi}$.

of more than 2000 years, led Napier to one of the most valuable and most useful inventions of modern science.

The result of the calculation, carried on in this ingenious manner, was most striking. A vast sphere, far in size exceeding the grasp of man's common apprehension, was to be supposed to be filled with particles so small, that their diameters were less than the $\frac{1}{1100}$ part of an inch. To attempt an enumeration of them had appeared perfectly visionary, and yet Archimedes shews that their quantity would fall short of 1000 myriads of his eighth order of numbers. He was aware that all this, so clearly and so easily expressed, would not carry conviction to the ignorant, but he knew that his conclusion must be admitted by the mathematician. He was conscious not only of the difficulty with which he had had to contend, but of the value of the means which he had devised to overcome it; but he simply concludes by saying *ᾠήθην ὅτι καὶ τινὰς οὐκ ἀναρμοστὸν εἴη ἔτι ἐπιθεωρῆσαι ταῦτα*: but none were then to be found who could follow out his views;—

> *Οὐ γάρ τις μέτα τοῖος ἀνὴρ ἐν τοίσδεσι πᾶσιν*
> *οἷος Ὀδυσσεὺς ἔσκεν.* Od. xxi. 93.

This sketch of the contents of the *Ψαμμίτης* can give but an imperfect idea of its value; and it is a short tract, which will well repay the time that may be employed in studying it. Hervagius first published it in the edition of the works of Archimedes, which he printed at Basil in 1544. His Greek text is indeed very imperfect, but there is in the same volume a Latin translation, which appears to have been made at an earlier date, and clearly from a much better manuscript than was used for the original language.

Commandine in 1558 gave us another Latin translation, but he appears not to have had a good manuscript for the foundation of his work. Rivaltus, in 1615, published a collection of the works of Archimedes, containing the present tract both in Greek and Latin; but he was confessedly unequal to the work which he had undertaken. Indeed the task was one of considerable difficulty. There is among the earl of Macclesfield's manuscripts, at Shirburn Castle, an original letter, in which Collins mentions that Dr. Barrow, when publishing the other works of Archimedes, at first intended to have omitted the Arenarius on account of the corrupt state of the original text, which he had not time to correct; and there was really no critical edition of this treatise, until that of Wallis was published at Oxford in 1679. It is reprinted in the third volume of his works, with not only his notes and translation, but with the useful addition of notices at the foot of each page, to mark the variations which had been made from the Basil text. It is a valuable work, and Torelli followed it in preparing this part of his edition of Archimedes for the press. Indeed he professes to take it for his standard: he departs from it in very few instances, and those few will be diminished nearly by one half, if we except two of them; —Wallis prints as two words, *ἀνὰ λόγον*, what Torelli unites into one, *ἀνάλογον*, and he prints *φανηρὸν* where Torelli, with all the manuscripts and all the printed editions, reads *φανερόν*.

In Ballard's collections in the Bodleian, there is an unpublished letter of Dr. Wallis to Dr. Charlett: it has no date, but from the handwriting it is clear that it was drawn up in his old age. "That Archi-

“ medes of Syracuse,” it says, “ did write in the Doric
“ dialect, we need not question. All that is left
“ of his, with which Eutocius hath not meddled,
“ remains in the Doric dialect to this day; namely,
“ that De conoidibus et sphæroidibus, that De lineis
“ spiralibus, his Arenarius, and his Quadratura parabolæ
“ ......That De sphæra et cylindro, and that
“ De dimensione circuli, on which two Eutocius
“ hath thought fit to leave us his large commentaries
“ for the better understanding of them, are now in
“ a dialect very different from the Doric of Archimedes.
“ For Eutocius, it seems, thought fit (for the
“ same reasons) to change the text also into the same
“ dialect with that of his comment, as more common
“ than that of the Doric.” Now Wallis, in revising the text, paid great attention to restoring the Doric forms of spelling and accentuation; and the introduction of *φανηρὸν* is certainly systematical and intentional, but no reason is given for it. Maittaire notices this variation; but as he only refers to Wallis's own book as his authority for its existence, he does not supply any additional argument for the alteration[c].

Torelli omits the whole body of Wallis's notes, and merely gives at the foot of his pages some various readings: from these it appears that he had the advantage of a MS. although he has made little or no use of it. He most probably was not aware that another source of elucidation existed in a superior text of the Ψαμμίτης, which had been printed in Great Britain about the middle of the last century. It was never indeed published, and only a limited number of copies got abroad: these likewise were without any title page, or any notice to indicate from whence they came, or under whose care they had been edited. It is probable that the whole history of them would by this time have been lost, if Mr. Barnwell, of the British Museum, had not preserved it, by inserting the particulars in the copy belonging to the library of that establishment. From him we learn, that the book was prepared for the press by Dr. Moor, who was professor of Greek at Glasgow, and who contemplated a complete edition of the works of Archimedes. What may have interfered with the execution of his intentions is not known, but this part was printed in small folio by Foulis with his usual excellence, and the text was taken from a manuscript which was lent to the editor by the Abbé Salier, as it appears, out of the French king's library. When the Oxford edition of Archimedes was published in 1792, various readings were collected from the continent, and added in an appendix: for this purpose collations were procured from four manuscripts in the Royal library of France, [No. 2359, 2360, 2361, 2362,] but none of them answer to Dr. Moor's edition; and Mr. Barnwell tells us, that in 1828, after a diligent search at Paris, he could find no traces of any manuscript which would agree with it[d]. The difference of this from all other editions is very remarkable, its Greek is often much superior to them, but nothing was known till 1830 of any place, in which an original authority was to be found for what had been thus adopted. In the beginning, however, of that year, Mr. Powell of Balliol died, and left his mathematical books to the Oxford Observatory, among which was a MS. of the Ψαμμίτης, in which Mr. Barnwell immediately recognised the readings that distinguished Dr. Moor's edition. A careful collation proves the resemblance to be so strong, that the first impression conveys the idea of it being the Paris MS., but still there may be some room for doubt. Mr. Barnwell mentions the copy of a letter from Dr. Moor which bears immediately upon this point. It is written in French, dated "26 Juillet 1751 N. S." and indicates that it was to be sent by the post to the Abbé Salier, apprising him of Mr. Foulis being about to set out immediately for Paris, when he would restore "le petit MS. du traité d'Archimède." Now we have no knowledge of any impediment to its arrival at the place of destination, and the precaution taken in sending previous notice of its return, would of course occasion inquiries, if it had not been delivered. Mr. Powell was first of Brase Nose, to which his friend Mr. Hindley belonged, by whom the MS. was given to him; but Mr. Hindley unfortunately became lost to

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[c] Sturzius in his republication of Maittaire's Dialects, makes no addition in this part, (p. 482. C.) Wallis prints *φανερὸν*, in the usual manner, in his edition of Aristarchus Samius &c. No notice is taken (Wallisii Opera III. p. 519, &c.) of *φανηρὸν* as a variation from the Basil orthography, although alterations of much less importance are duly pointed out.

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[d] It may be remarked likewise that the Greek mathematicians are contained in the second volume of the Cat. Codd. MS. Bibl. Regiæ Par., which was published in 1740 with such great shew of attention to making it complete that an appendix is added at

p. 609, "complectens Codd. MSS. Constantinopoli paucis abhinc mensibus in Bibl. Regiam illatos." Montfaucon likewise, in his Bibliotheca Bibliothecarum Manuscriptorum, enumerates only four Greek MSS. of Archimedes in the French king's library, (No. 2153, 4, 5, 6,) which seem to answer to those mentioned above.

society in 1802, and has been dead many years: his friends can give no information of the channel through which the MS. came to his hands, and all is therefore left to conjecture. The book is indeed a small thin volume, but this is not conclusive, since for any short treatise like the present, the transcriber would have hardly been inclined to take a larger page.

The present bishop of Peterborough once endeavoured to devise a formula[d], by which a MS. might be identified from the readings which were found in it; but, as Mr. Porson remarked[e], "a multitude of "considerations are necessary to form a correct "judgment on MSS. which cannot possibly be re"duced to any theoretical computation," like that on which Dr. Marsh's theorem was founded. In the history of printed books, Ruddiman is said to have published editions of the classics without the deformity of a single press error, and the Glasgow Horace of 1744 has the reputation of being immaculate. Such excellence, however, is the result of repeated revisions; the late Henry Homer never hesitated in cancelling any leaf in which an erratum had been discovered. Now this could hardly have been done, at least to any great extent, in the construction of a MS. Corrections might be and certainly were sometimes introduced, but we are not certain of the care having been taken for other transcripts, which has been so wisely used in examining those of the Hebrew scriptures. But although many copyists must have worked for bread, and, from their haste, have been liable to great inaccuracies, others in monasteries could proceed with more leisure, and may have executed their task with more fidelity. The Oxford MS. is not considered of an earlier date than the 15th century[f], and it is impossible to say that the prototype from which it was taken, or others from the same source, may not now be in existence. None indeed of these are known; but no more was this very book till within these last few years: a number of its best readings coincide with the expressions in the old Latin published by Hervagius;—are we from thence to conclude, that it was the identical MS. from which that translation was made? This could not be maintained, and if it can not, these readings have existed in some other MS., which may yet be brought to light.

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[d] Letters to Archdeacon Travis, p. 70.

[e] Dr. Milner's Strictures on some of the publications of the Rev. Herbert Marsh, D. D. p. 252.

When an allowance has been made for the restoration of Doric forms and for the corrections of obvious errors, the number of places is very small in which Dr. Moor's text differs from that of our MS. In the copy of his letter to the Abbé Salier, he mentions that a new type was to be formed at Glasgow from that of the Louvre Terence, and that he was waiting for it in order to print a Latin translation of the work. He speaks also of the notes which he intended to annex to the whole. But nothing has been met with to indicate his having executed these parts of his design, and it is impossible to tell what alterations he may have adopted entirely from his own critical conjectures. Wallis, who confessedly had no other means of correcting the text, has suggested many emendations, which are now confirmed by the readings in the Oxford MS. Some allowance, therefore, must be made in reasoning from this similarity. A few discrepancies, which cannot be otherwise accounted for, may have more weight in the opposite scale than a number of instances in which a sameness may be found to exist. In the 4th page of Dr. Moor's edition the words [*διάμετρος ἁλίου*] are printed between brackets. Now these words are neither wanting in the Basil, Rivaltus, or Wallis's editions, nor are they either parenthetical or redundant[g]; it is difficult therefore to imagine why they should have been marked in this manner, unless it were to indicate that they had been inserted in this place without the authority of the MS. from which the text was taken; and they are not deficient in ours. But there is another which seems to be a stronger case: Archimedes, when desirous of assuming some definite measure, which may be less than that of the grains of sand, derives his first quantity from the number of poppy seeds, which, laid beside each other in a straight line, would make up a *δάκτυλος* in length. The Basil Greek makes this 25, but the old Latin and Commandine say 35; Wallis therefore with good reason conjectures that κ̅ε̅ has been written by mistake for λ̅ε̅, because Archimedes, in his endeavour to take the argument *a fortiori*, immediately says that he shall consider them as not more than 40, which is the next round number above the larger of these quantities. Now Dr. Moor in this place has *εἴκοσι ἓξ* in words, and he could have no inducement to introduce an alteration of the units; for the argument in favour of $\overline{\lambda}$ for $\overline{\kappa}$ will be as strong against 26 as against 25. There can be no reason given for what he has printed, but the supposition of his having found it so in his MS., and if he did, it was not the same with ours, which distinctly reads *κε*, or 25, in numerals.

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[f] It contains an ancient form of the beta u (vid. Montfaucon Palæog. Græca p. 336,) but the use of it is confined to the word ἑβδόμος. This may be the trace of an old MS. from which it was transcribed. Nothing more can be deduced from it, since, for other words, the common form of the letter is constantly occurring.

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[g] There is not a single parenthesis introduced in the whole of Dr. Moor's text. One indeed is omitted at p. 16, which stands (δέδεικται γὰρ......τῷ κύκλῳ) in the Oxford MS.

The Quarterly Review[h], in an account of Peyrard's Archimedes, tells us, that "the most valuable "edition of the Arenarius is that published in Eng-"lish at London in 1784, by Mr. George Ander-"son," and that it "deserves notice, not only as the "first edition of any part of Archimedes' works in a "modern language, but on account of its useful and "ingenious notes and illustrations. It is followed "by a translation of the Latin dissertation of Chris-"topher Clavius on the same subject, and is alto-"gether so valuable, that we cannot but regret that "it has become scarce." It is by no means accurate to describe this as the first translation from Archimedes into a modern language; for the treatise, "De iis quæ humido vehuntur," was published in Italian by Tartaglia in 1551, in French by Pet. Forcadellus in 1565[i], and in English, as a part of his Mathematical Collections, by Salusbury in 1662[k]. J. Ch. Sturm likewise published a German translation of the Arenarius in 1667, and of several of the works of Archimedes in 1670. All this, however, in no way diminishes the value of Anderson's work, which, though it would admit of improvement, is still possessed of considerable merit.

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[h] Vol. iii. p. 92.

[i] Fabricii Bibliotheca Græca a Harles, vol. iv. p. 177. Petrus Forcadellus was probably Pierre de Forcadel de Beziers.

[k] In the General Dictionary by Bernard, Birch, and Lockman, it is said in a side note (vol. v. p. 372.), with respect to

Anderson entered at Wadham college in March 1780[1], and was appointed Bible clerk in the following July. He was the son of a small farmer of Weston Turvill in the county of Buckingham, and arrested the attention of all who knew him by his early and self-taught acquirements in the mathematics. By the benevolence of the vicar of Whitechurch, near Aylesbury, he was not only sent to college, but was in great measure supported during his residence in Oxford. After proceeding to his bachelor and master's degrees, as soon as he was of standing for them, he took deacon's orders, but appears to have advanced no further, nor to have long directed his thoughts to the pursuits of the profession. His kind friend, the rev. Mr. King,

Salusbury's Collections, that "almost the whole impression of "the second tome was destroyed in the fire of London in 1666. "The only copy perhaps now extant is in the curious library of "Will. Jones, esq. F. R. S." Mr. Drinkwater, in his Life of Galileo, (p. 104,) says, that "only one copy is known to be ex-"tant in England." He quotes Chauffepié for his authority, probably not being aware that this French writer merely translated in 1750 what had been published in England by Bernard and his coadjutors in 1737. The translation of Archimedes makes a part of the second tome; but having been printed four years previous to the latter part of the Collections, some copies of it got abroad before 1666: one is in the Bodleian. Still, however, they are very scarce, and may easily have escaped the reviewer's notice.

[1] Many of the following particulars are taken from an account of Anderson in the Monthly Magazine, vol. i. p. 345.

afterwards recommended him to Mr. Scrope Bernard, and by the assistance of lord Grenville he obtained employment under government. He is said in the Quarterly Review to have been confidential clerk to the board of control under Mr. Dundas. His real place was "accountant" in that office, and he obtained it about 1787, but he certainly was entrusted by ministers with other confidential business; since his anxiety to complete some calculations for the budget of 1796 made him neglect, till it was too late, a disease which in consequence ended his life within a few days. When this took place he was only in his 36th year; and the president of the board, in a letter of condolence to his widow, described him (apparently with great truth) "as a public loss."

He is said to have made himself well acquainted with the affairs of the East Indies, and to have drawn up a valuable work connected with them; but, with this exception, he seems to have published nothing of any magnitude but the translation from Archimedes. He evidently knew little or nothing of the learned languages before he came to Oxford, and he then applied himself to them rather as a duty than as a source of intellectual gratification; but he was endowed by nature with strong talents, and his proficiency obtained for him one of the Hebrew exhibitions which Dr. Hody had founded in his college. He was enabled to pass the examinations, which he had in consequence to undergo from the regius professor of that language, but still he must have felt that his acquirements in literature were not what he had most to rely upon. His translation of the Arenarius was published about the time of his taking his degree, and, under all the circumstances of the case, does him great credit; but it must be looked upon as the work rather of the mathematician than of the scholar and the critic. He probably had no means of making himself acquainted with some unpublished remarks on the work by Dr. Wallis, which are preserved in the Savilian library; but he speaks[m] of a copy of the Basil Archimedes in the Bodleian, "with MS. corrections by Dr. Bernard," which he could not have examined; for if he had, he would have found that for the Ψαμμίτης they contained little more than a partial insertion of some readings adopted by Rivaltus. These, indeed, could have been of no service to him; but he would have gained a great advantage, if he had not, from an excess of caution, declined to make any use of Dr. Moor's edition, to which nevertheless he frequently alludes. The form in which it got abroad was certainly calculated to excite hesitation, and as he found no information of the authority for the variations in the text, he would have been right to have considered well before he admitted them; but he should have recollected that Wallis's emendations were all conjectural, and even if the later edition had been corrected in the same manner, it became merely a critical question to determine which of the editors had suggested the more probable readings. He explains his reasons in a note[n] on the first page of his book. He points out, that in the very second sentence of the treatise, there is a variation in the old Latin of the Basil edition from the trans-

[m] Page xiv.

[n] Page 33.

lation which he has given; and he goes on to say, " with this (Latin) the anonymous Greek edition of " the Arenarius alone agrees, having instead of λέγω, " ἐγὼ, and adding, to render the sentence complete, " πεπερασμένον φαμὶ εἶμεν. But as there appears no " other authority for so material an alteration in " the Greek text and its signification, that meaning " is here given which seemed to agree best with " the more ancient Greek copies, and which is also " given in the Latin translations of Paschasius, " Commandine, Rivaltus, and Dr. Wallis." Now the title of the work published by Paschasius Hamellius is only, " Commentarius in Archimedis li- " brum de Numero Arenæ;" and although he states the parts of the book as introductions to his several remarks, he does not always give complete and exact translations of them; his expressions, therefore, are only of secondary use in settling the text. Commandine, as has been already mentioned, translated from a bad manuscript, and Wallis says of him [o], that in some places " sensum potius sequitur " quam verba." Of Rivaltus it will not be severe to assert that he can never be depended on; and Dr. Wallis for himself says [p], " Hervagius, cum vi- " deatur etiam ea loca, quæ manifesto erant depra- " vata, ita edidisse ut in manuscripto codice reperta " fuerant, nobis interim integrum reliquit, per veri " quæ supersunt vestigia, nostras etiam conjecturas " interponere, absque παραδιορθώματος præjudicio." His main foundation rested entirely on the imperfect text of Hervagius's edition. As far as authority therefore is concerned, very little from the formidable array of Mr. Anderson can be opposed to the better sense which may be found in the Basil Latin. He seems, however, to have unfortunately taken a very mistaken view of this part of the subject. He says[q], that the "anonymous edition of the Arenarius seems "to have been altered on purpose to agree with "the (old Latin) translation." It is now certain that this suspicion was inaccurate; but if it had not been so, still it was no reason for the total rejection of the edition. Its variations from Wallis's are generally for the better, but in many places they go beyond alterations of punctuation, or mere verbal corrections. The adoption of the words in which they are expressed would have required mature deliberation in one who was about to publish the original Greek, but the case was widely different for an English version[r]. It would have been fair in the first instance to presume that such material alterations were not wholly arbitrary; when set, as here stated, at the very lowest estimate, they shewed the editor's confidence in the old Latin translation; and as Mr. Barnwell has well remarked, if Mr. Anderson's objection "were admitted, there still remains the question, from what source did the "translator derive his readings?" His work, as Mr. Anderson himself observes[s], "seems to have "been written long before the publication of the "Greek text;" ("Latinitate jam olim donata, nunc-"que primum in lucem edita," are the precise words of its title-page;) it could, therefore, have been only made from an original manuscript, and it is of equal, if not of more authority, than the Greek editions on which Anderson relies. He speaks indeed of "more ancient Greek copies," but all their claim in this respect can only depend upon their having been printed at an earlier period than Dr. Moor's, which is very far from proving that they rested upon more ancient authorities.

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[o] Introduction to the notes.

[p] Ibid.

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[q] Page xv.

[r] It may be remarked, likewise, that Mr. Anderson was not very scrupulous in adhering closely to his original, when he thought that he could improve it. Archimedes, after having laid down the absolute quantity in stadia, beyond which he concluded that the diameter of his universe could not extend, goes on to find a magnitude which the grains of sand would not exceed in minuteness. He then, after explaining his system of numeration, proceeds to calculate how many of such magnitudes would be required to make up spheres of different dimensions. Now Mr. Anderson entirely omits the first estimate of the minute grains of sand from its proper place, and interweaves the particulars of the passage with the other second part, in which the calculation begins. It possibly did not strike him, that he was by these means violating the clear method which Archimedes had adopted: and without obtaining any real advantage, he has thus departed from that faithfulness with which a translator ought always to endeavour to exhibit the writings on which he is employed.

To the old translators of the Greek mathematicians, the scientific world is under the greatest obligation. Campanus, in 1482, published Euclid's Elements in Latin from the Arabic, fifty years before that excellent work appeared in its original language; a very curious treatise of Archimedes on floating bodies is preserved to us in Latin, now that the Greek manuscript from which it was translated is no longer known to exist; and the Mathematical Collections of Pappus, one of the most valuable works which issued from the school of Alexandria, are, for the most part, only accessible to the general student in the Latin of Commandine. This last would be admitted as of good authority, if any one undertook to bring out the book in a modern language, and it affords essential service even in correcting the Greek, which is found in the manuscripts. But independent of the advantage which can be derived from such works, (which really are so many perpetual comments on the respective authors,) the case of Barocius's translation of Proclus may be adduced as a strong proof that the Latin sometimes is much more worthy of confidence even than what is found in the original Greek. This likewise is immediately in point; for Hervagius, the same printer who published the Basil Archimedes, published at the same place, in 1533, this Comment of Proclus on the first book of Euclid. The work is disfigured by the speculations of the Pythagoreans and later Platonists; for, as Bacon observes[t] of this very author, "hoc habet ingenium humanum ut cum ad "solida non sufficiat, in supervacaneis se atterat." Still, mixed up with these fanciful notions, many things are recorded of singular curiosity. Among other notices, it preserves an anecdote, the interest of which may be well estimated by its being so universally known. It will likewise give one of the very many instances in which the original Greek is defective. In speaking of Euclid he says, *μέντοι καὶ φασὶν ὅτι Πτολεμαῖος ἤρετο αὐτὸν εἴ τίς ποτὲ ἐστι περὶ γεωμετρίαν*. This broken and enigmatical sentence is all that the original affords; but Barocius, whose Latin translation appeared in 1560, fills up the deficiencies in general, and gives the corresponding passage with complete sense: "Quinetiam ferunt "olim Euclidem a Ptolemæo interrogatum, essetne "aliqua ad geometriam capessendam elementari "institutione brevior via, respondisse nullam esse "viam regiam, quæ ad geometriam ducat." Now the mutilated Greek text is not the fault of Grynæus, who was the editor of it, but must be attributed to the state of the manuscript from which he printed. In the preface, addressed to Cuthbert Tonstall, he says, "Mihi ipsi Procli commentaria "Oxonii Joan. Claymundus candide suppeditabat." John Claymond was the first president of Corpus Christi college from 1516 to 1537; and there can be little doubt of his being the person here mentioned, and having supplied the text from a MS. still preserved in the library of that college, in which this passage occurs precisely as it is printed in the Basil edition[u]. Fabricius, in mentioning the Latin of Barocius, says, "Operæ pretium fecisset si "contextum addidisset Græcum," and it seems extraordinary that he did not; for in his dedication he justly remarks, on the commentaries as printed at Basil, "Adeo laceros et corruptos inveni, ut nihil "boni ex eis elicere potuerim:" but we have his own Greek MS. in the Bodleian, in which we find the authority for his corrections; and from thence we learn the true reading of the sentence just referred to. *Βασιλικὴ ὁδὸς* appears to have been no unusual expression with other writers; and as there has been no second edition of Proclus, it has naturally been adopted whenever allusion is made to this passage; but Euclid told the king in still more terse and definite language, [x] *μηδεμίαν εἶναι βασιλικὴν ἀτραπὸν ἐπὶ γεωμετρίαν.*

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[s] Page xv.

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[t] De Augment. Scient. lib. iii. cap. 6.

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[u] The Corpus MS. also, like the printed book, has no diagrams; and there is another coincidence which is still more remarkable. In p. 24. l. 5. of Grynæus we have *τὸ οὖν ἰσόπλευρον μὴ εἰς ἄπειρον, κ. τ. λ.*; but between *ἰσόπλευρον* and the following word *μὴ*, it appears from the Barocian translation and MS. that there is wanting as much of the Greek text as would possibly occupy a full folio page.—There is exactly the same omission in the same place in the Corpus MS.

[x] In the text of the MS. the word is μ̈η, which Dr. Gaisford has adopted in his edition of Stobæus, (vol. iv. App. p. 45. note [z],) where he gives the whole passage; but in the margin of the MS., against the same line, we find μ̈ιαν, which is probably intended for μηδεμίαν, as Barocius translates the word by "nullam."

⁂ Some mistakes having been made with respect to the measurement of the sun's diameter by Archimedes, it may be useful to clear up the obscurity which they have introduced into that part of the treatise.

For the fundamental observation, he says, *τεθέντος μακροῦ κανόνος ἐπὶ ἐπίπεδον, ὀρθὸν ἐν τόπῳ κείμενον ὅθεν ἤμελλεν ἀνατέλλων ὁ ἅλιος ὁρᾶσθαι· καὶ κυλίνδρου μικροῦ τορνευθέντος καὶ τεθέντος ἐπὶ τὸν κανόνα ὀρθοῦ, εὐθέως μετὰ τὰν ἀνατολὰν τοῦ ἁλίου· ἔπειτα, ἐόντος αὐτοῦ ποτὶ τῷ ὁρίζοντι, καὶ δυναμένου ἀντιβλέπεσθαι, ἐπεστράφη ὁ κανὼν ἐς τὸν ἅλιον, καὶ ἁ ὄψις κατεστάθη ἐπὶ τὸ ἄκρον τοῦ κανόνος· ὁ δὲ κύλινδρος, ἐν μέσῳ κείμενος τοῦ τε ἁλίου καὶ τᾶς ὄψιος, ἐπεσκότει τῷ ἁλίῳ· ἀποχωριζομένου οὖν τοῦ κυλίνδρου ἀπὸ τᾶς ὄψιος, ἐν ᾧ τόπῳ ἄρξατο παραφαίνεσθαί τι τοῦ ἁλίου μικρὸν ἐφ᾽ ἑκάτερα τοῦ κυλίνδρου, κατεστάθη ὁ κύλινδρος.*

This text differs but very little from that of Dr. Moor. Hervagius has *ἐπὶ τὸν κανόνα ὀρθοῦ*, but Rivaltus prefers *ὀρθροῦ*, which, as Dr. Wallis remarks, "sensum pervertit." The MS. C, however, of the French king's library, which was collated for Torelli's edition, has *ὀρθροῦ* in the text, and the MS. B has it as a marginal correction; the Oxford MS. has likewise the same reading, and Dr. Moor prints the passage, *κυλίνδρου . . . . τεθέντος ἐπὶ τὸν κανόνα, ὀρθροῦ, εὐθέως μετὰ τὰν ἀνατολὰν τοῦ ἁλίου.* Now it is difficult to conceive any good reason why the cylinder was to be applied rather at this particular time than at any other previous to the observation. This applies to both the readings; but there is a contradiction in speaking of the "twilight, immediately after (or even at) the rising of the sun." There can, therefore, be no doubt of this reading being wrong, and that the original word restored by Dr. Wallis is right.

The *κανὼν* was a straight rod, which having been placed flat against the side of an upright plane, a well turned cylinder was contrived to move along it at right angles to that plane: this was placed at first so near the eye as entirely to cover the sun's disc, and then gradually removed till a small quantity of the sun's light became visible on each side of it.

Paschasius Hamellius saw accurately from this description, that Archimedes observed the perpendicular, and not the horizontal diameter of the sun. It was the more easy to observe, and Archimedes might not have been aware of the error which was thus occasioned by the difference of refraction on the upper and lower limbs; or having marked the oval form of the orb on the horizon, he may have taken the shorter diameter in order to give the greater force to his argument. His object was to deduce the sun's distance from its assumed magnitude and apparent diameter; consequently the less the observed angle, the greater would be the radius of his *κόσμος*.

Hervagius begins the description by *τεθέντος μακροῦ κανόνος ἐπίπεδον*, and the old Latin translation renders it "posita longa regula super planum erectum;" but Rivaltus, finding nothing to govern *ἐπίπεδον*, proposes to divide the word into two, *ἐπὶ πέδον*, and confuses the whole by supposing that the cylinder lay flat on an horizontal table, which was placed upon an upright stand. Dr. Wallis saw that this position was objectionable, and incompatible with the reading which he adopted; but the deficiency of grammatical construction induced him to admit the conjecture of *ἐπὶ πέδον*, in which he was certainly mistaken. "Super fulcrum" is the sense which he gives to the words; but *πέδον* is a distinct root, not a derivative from *ποῦς*; it means the ground itself, not a support, which may raise any thing above the surface. He says also, "erigendus cylindrus est ad regulam normalis," which seems to indicate that he considered the cylinder to be used for measuring the horizontal diameter of the sun, and this appears to be the sense in which Mr. Anderson has understood the passage, but there can be no doubt that all difficulty and ambiguity are best removed by Dr. Moor's reading, which agrees with that of the Oxford MS. Nothing can be more easy to conceive than that the transcriber overlooked the first in the consecutive repetition of *ἐπί*.

In a subsequent demonstration Archimedes takes the case when the sun has risen above the horizon, and argues from its centre being then further from that of the earth than from the place of observation. Nothing can be more clear and simple than this truth, as derived from the diagram commonly used for the illustration of parallax in altitude; but Dr. Wallis has thought it necessary to add an explanation, in which he deduces this fact from the obtuse angle which will be made by the line drawn from the eye of the observer to the centres of the sun and the earth, which he says must be so "propter solis diametrum majorem quam telluris." Now the annexed reason only creates a difficulty. It is hard at first to see what difference can be supposed to exist in this respect between the sun's centre and a fixed star; but the error is derived from Commandine's commentary, where he introduces the case of the sun being on the rational horizon, and says, that as its semidiameter is greater than that of the earth, its centre would then be raised above the sensible horizon. Now there is no reason whatever for this refinement. Archimedes argues from the angle which he observed the perpendicular diameter of the sun to subtend, and even if any doubt could remain of his meaning, it must be removed by his precisely representing the position of the horizon, in his diagram, by a line which is a tangent to the earth's surface at the point where the observation is supposed to be made.

ON THE

NATURE AND EVIDENCE

OF THE

PRIMARY LAWS OF MOTION.

BY THE

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The Ashmolean Society desire it to be understood, that they are not answerable, as a body, for any facts, reasonings, or opinions, advanced in papers printed by them.

It appears by the Reports of the Ashmolean Society, N°. III. that on Feb. 7, 1834, the Author read a paper having the same title as the present. That paper constituted a portion of what is here presented, viz. that part relating to the second law of motion. He has for a long time had the essay lying by him, but has not been able until lately to complete the other portions of it; he has thus, however, been able to avail himself of much which has been recently written on the subject; and the whole, after a careful revision, is now submitted to the Society.

"This I say to encourage those who are not far gone in these studies, to use intrepidly their own judgment, without a blind or mean deference to the best of mathematicians, who are no more qualified than they are to judge of the simple apprehension, or the evidence of what is delivered in the first elements of the method." *Bp.* Berkely, *Defence of Freethinking*, &c. p. 21.

"If on a plain case I appear to argue tediously and unnecessarily, let it be recollected that I am only paying a tribute, which is always exacted, when an opinion, long received or sanctioned by great authority, is controverted." *Prof.* Woodhouse, *Anal. Calc.* pref. iii.

"Ἔστι δὲ ταῦτα τοῖς φυσικοῖς προβλήμασιν οὔτε ταὐτὰ πάμπαν οὔτε κεχωρισμένα λίαν, ἀλλὰ κοινὰ τῶν τε μαθηματικῶν θεωρημάτων καὶ τῶν φυσικῶν· τὸ μὲν γὰρ ὧς διὰ τῶν μαθηματικῶν δῆλον, τὸ δὲ περὶ ὃ διὰ τῶν φυσικῶν." Aristot. *de Mechanica*, §. 1.

ON THE

NATURE AND EVIDENCE

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PRIMARY LAWS OF MOTION.

READ TO THE ASHMOLEAN SOCIETY, NOV. 20, 1837.

Introductory Remarks.

PRIOR to the discoveries of Galileo, the only speculations on dynamical subjects were those of the peripatetic philosophy. In the writings of the great founder of that sect a few elements of mechanical truth shine forth with conspicuous brightness, but in the interminable disputations of his successors they were soon lost and disguised in an overwhelming admixture of metaphysical subtilties; and a number of absurd conclusions, deduced from the most vague and arbitrary assumptions, or rather, verbal quibbles framed out of the most unmeaning abstractions, were held forth as the realities of mechanical science. A spirit of metaphysical theorizing, scarcely less unsubstantial, characterized the Cartesian school of a succeeding age; and notwithstanding the advances of sound inductive science, some tinge of this scholastic mysticism continued till a much later period to infect the language, if not the ideas, of philosophers; and perhaps the prevalence of such views, sanctioned by the authority of great names, has, even to the present day, tended to involve certain parts of the science in unnecessary obscurity; and none more so than the exposition of the first rudimentary principles of dynamical investigation, and the reasoning by which they are established.

When the doctrines of force and motion had been brought under the dominion of mathematical analysis, those who undertook to treat the subject systematically were soon led to attempt to reduce its first principles into the same methodical train as that in which the elements of geometry had been delivered. Following the model of the geometrical treatises, they began by formally embodying certain primary truths in the shape of distinct fundamental elementary propositions. These (whether or not they were properly axiomatic, or even necessary truths) were at least considered to bear the same relation to the rest of the science as the axioms did to the theorems of geometry. They consisted of a few general principles, which were regarded as the simplest elements into which all the cases of mechanical action could, by ulterior analysis, be resolved, and from which all mechanical theories were, on the other hand, to be synthetically deduced, and were designated as "the laws of motion."

Assumptions of this kind, equivalent to what have been since called the first two laws of motion, are implied in several parts of the reasonings of Galileo; perhaps rather regarded as hypotheses, than propounded as certain truths. But in his dialogues on mechanics, 1630, the first law is formally enunciated: the second law is also involved in his reasoning on falling bodies and projectiles. Several among his followers, and especially Gassendi, gave experimental confirmations of them, particularly of the second law. Borelli (1667) stated distinctly the first law. Wallis referred to the first law as hitherto commonly adopted, as a *postulate* among mathematicians, and remarked the want of a demonstration of it. Huyghens had laid it down as a hypothesis. Des Cartes proposed, as the foundation of his system, three laws of nature (as he terms them) in regard to motion, which he attempts to prove in the usual *a priori* method of his philosophy. Hooke, in his endeavours to explain the planetary motions, among his assumptions, formally laid down one which is nearly the same as the first law.

Newton, in the introductory portion of his Principia, has given, in accordance with the views of the writers who preceded him, and not impossibly (as has been imagined), upon the suggestion of the three laws of Des Cartes, a statement of those three primary principles, which he entitles "Axiomata, sive leges motus;" and in the scholium upon them expressly acknowledges "hactenus principia tradidi a mathematicis recepta," instancing two of them as having been recognised by Galileo as the basis of his reasoning; thus clearly disclaiming originality in the discovery of them.

The three laws of motion (almost exactly in the form given by Newton) have been generally stated thus: First, "that a body in motion will move uniformly in a straight line, unless acted on by some extraneous force. Secondly, that change of motion is proportional to, and in the direction of, the force impressed. Thirdly, that action and reaction are equal and in opposite directions."

The laws stated in these, or nearly equivalent terms, have been received and adopted by almost all English writers till very lately; and have been upheld as an exact statement of the simplest principles to which we can ascend in our analysis of mechanical action, and from which, on the other hand, all the synthetical demonstrations of theory must originate.

But with few exceptions these writers have not been particularly scrupulous in canvassing the logical accuracy of the first rudiments of the methods they employ. They seem chiefly eager to hasten on to those points where the applications of mathematical science are to be introduced, and the analysis of the ultimate elements of physical truth, and of the exactness of the reasoning by which we arrive at them, are points which the mathematician seldom feels much interest in examining very closely. Perhaps, indeed, that peculiar turn of mind which confers the ability and the taste to pursue the higher kinds of mathematical investigation, is far from being the same as, or even allied to, that which creates the qualifications for a critical discrimination of logical accuracy: and a minute attention to the precision of definitions and hypotheses at the outset of an inquiry is often considered beyond the province of the mathematical writer, and as involving metaphysical questions from which he should endeavour as quickly as possible to disentangle himself.

There have appeared however, especially in recent times, some writers of a different stamp, who have entered largely into the discussion of these first principles. The question has been agitated whether these laws of motion are to be regarded as *axioms*, or only as admitted *assumptions;* as *necessary* truths, or only as general expressions of the simplest *laws*, which by a careful analysis of all mechanical actions can be derived from *observation* and *experience*. And again, whether three is the smallest number to which they are reducible, or whether they are susceptible of further analysis and simplification, so as to be included in still fewer propositions.

With respect to the former of these questions we may observe, that though Newton subjoins to each of the laws certain examples taken from known facts in mechanics and dynamics, yet it does not clearly appear whether he intended these instances to be regarded as the actual *proofs* of the propositions, or merely as *illustrations*. He certainly speaks of these laws in the scholium as "experientia multiplici confirmata;" and mentions several such instances, particularly in support of the third law. It may also be remarked, that in his Optics he applies the same title of axioms to the laws of reflection and refraction, which unquestionably are purely inductive results.

In this discussion the continental philosophers have borne a conspicuous part; they have canvassed very closely the Newtonian laws, and the reasoning by which they are supported. Euler and D'Alembert have given investigations of them of an abstract mathematical kind, chiefly on the principle of "sufficient reason." The later French writers, especially Laplace and Poisson, reduce the three laws to two; at the same time contending that their truth depends wholly on induction. "Voila donc," says Laplace, [Mec. Cel. liv. i. c. ii. §. 5.] "deux lois du mouvement, savoir la loi d'inertie et celle de la force proportionelle à la vitesse, qui sont données par l'observation.

"Elles sont les plus naturelles que l'on puisse imaginer, et sans doute elles dérivent de la nature même de la matière: mais cette nature étant inconnue elles ne sont pour nous que des faites observés: les seuls, au reste, que la mécanique emprunte de l'expérience."

Among British mechanical philosophers, Professor Playfair was one of the first who ventured upon any departure from the letter of the three laws of Newton, and professed to discuss the logical accuracy with which they were established. He reduces them to two: the first being the same as that of Newton, the second the same as Newton's third; a demonstration being adduced to shew that Newton's second is only a consequence from the first. Some parts of the subject were also discussed by Mr. Home (afterwards Lord Kaimes) and Dr. J. Stewart (especially the question of motion and inertia), in their papers in the Edinburgh Physical and Literary Essays, vol. i.

Dr. T. Brown, in his inquiry into the relations of cause and effect, has entered largely into the discussion of the fundamental principles of reasoning in mechanics, more especially with reference to the doctrine of "sufficient reason," appealed to, as we have observed, by some eminent philosophers for the demonstration of the laws of motion *a priori*. Dr. Brown gives at large a refutation of this principle, and contends that these laws depend entirely on experience for the evidence of their truth.

The whole subject is most ably discussed by Mr. Whewell, both in his treatises on dynamics, and in several memoirs. In a paper in the Edinburgh

Journal of Science (No. XV. p. 27.) especially, he examines at large the reasoning by which the second law is attempted to be reduced to a consequence of the third; whence he concludes, that the French attempt at simplification is fallacious. In his systematic treatises, giving the three Newtonian laws, stated in considerably different and more precise terms, and limited to the doctrine of motion (as distinguished from that of statics), he contends that they form the simplest elements and necessary first data of the science; and that they derive their evidence entirely from experiment.

Subsequently the same author has resumed the subject in a profound paper in the Transactions of the Cambridge Philosophical Society [(1834) vol. v. part ii.], which contains the most complete developement of his views.

He lays down, as the basis of his reasoning, three axioms, properly so called: first, that every change is produced by a cause; second, that causes are measured by their effects; third, that action is always accompanied by an equal and opposite reaction. The first two he proposes as truths absolutely and independently self-evident. The last as necessarily involved in our idea of body or matter, and of the action of one portion of it upon another. He then proceeds to state the laws of motion as consequent upon these axioms, in a form somewhat different from that in which they were formerly given. He considers their evidence as in one sense axiomatic, in another, inductive; that is, from these axioms we might deduce certain laws as necessary truths, but we could not apply them to the actual affections of matter without the aid of experience. Observation, however, shews us that these necessary abstract truths do actually apply to things constituted as they are: hence they become the safe and substantial basis of our reasonings.

These views are still further illustrated by the account of the progress of opinion and discovery relative to the first principles of motion, given by the same writer in his History of the Inductive Sciences. [vol. ii. ch. 1.]

With respect to the attempts to prove these laws *a priori*, it will perhaps be said at once that they are manifestly futile, it being easily evident that no conclusion relative to questions of *fact* can possibly be proved by *a priori* reasoning. (See Abp. Whately's Logic, p. 226.) But in the present case it would seem that even those who have gone furthest in supporting the metaphysical view of the subject, have all along tacitly admitted the necessity of *some* appeal to observation; they have at least assumed the elementary properties of matter, without which no such conclusions could apply to nature: in other words the term *a priori* is misapplied as descriptive of these arguments. The discussion then, in any case, will be found really reducible to the question, what are the *simplest* and *fewest* physical assumptions to which it is necessary to refer?

It has been from the study of the elaborate and valuable discussions of Mr. Whewell, that my attention has been principally called to this subject. But after the most careful examination of the several arguments, I feel obliged to confess myself not completely satisfied; and to think that there are yet some points requiring further elucidation before the subject is placed in such a light as entirely to free it from difficulty and objection. In the present attempt to contribute towards this object, while I controvert as fallacious many of the metaphysical refinements which have been introduced, and refer to observation solely for the first elements of all physical reasoning, it will be found that I endeavour to carry the process of simplification in those *physical* principles somewhat further than has hitherto been done.

The subject is in its very nature confessedly abstruse and dry ; and any critical discussion of the accuracy of statement and validity of proof in such elementary points will almost always appear of a repulsive character to those who are either, on the one hand, absorbed in the interest inspired by the applications of mathematical analysis ; or who, on the other, viewing the subject in its more practical relations, are anxious to hasten to results instead of canvassing principles ; or who (as bishop Berkely has well expressed it, in reference to a parallel case) " are men accustomed rather to compute than to think ; earnest rather to go on fast and far, than solicitous to set out warily and see their way distinctly." (Analyst. p. 17.) Such persons will perhaps regard all discussions of this kind as superfluous and trifling ; and, classing them along with the logical disputes of the schools, will reject them as metaphysical niceties, and of too speculative a character to hold any essential place in a system of inductive philosophy.

By those, however, who are interested in the subject in a more philosophical point of view, the value of inquiries into these topics will be better appreciated ; they will acknowledge the importance of questions which involve the logical soundness of the whole foundation of the science ; and will probably admit, on an examination of the whole subject, that it yet stands in need of an exact, careful, and systematic review; and that the field is still open to those inquirers into the elements of philosophical truth, who are anxious to see physical reasoning grounded on a sure basis of precise definition and logical deduction.

First Law of Motion.

THE first law of motion is stated by Newton thus: "Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum illum mutare." The meaning and extent in which he intends these expressions to apply is fairly to be collected from the illustrative examples which are immediately added.

Now these consist of the cases of projectiles, where the rectilinear motion of impulse is compounded with gravitation; of a top whose spinning is retarded, and at length ceases, from the resistance of the air; and, on the other hand, the planets, whose motion, from the absence of such resistance, continues undiminished.

The attentive reader of Newton readily perceives the sense in which these examples apply. The truth stated in such terms is sufficiently clear, and the instances referred to sufficiently in point for the author's purpose, which obviously leads to the cases of tangential and deflective forces.

The first law has been stated in nearly the same terms as those employed by Newton by other philosophers. D'Alembert has attempted a demonstration of it as a necessary truth from abstract principles. (Dynam. art. 6.) This proof turns upon reducing the question to one of the two following suppositions: The motion of the body is due either to the *indivisible* and *instantaneous* action of the moving cause at the commencement of the motion, or to its *continued* action.

In the first case, the space traversed after the instantaneous impulse *must* be uniform, because the body cannot accelerate or retard itself: and, in the second, there is nothing to cause the moving force to increase or to diminish, &c.

A more formal use of the same kind of reasoning has been made by Euler. He first argues that a body in motion, left to itself, cannot stop, since the same reason which would stop its motion now would have prevented it from moving at all. And in like manner, if originally at rest, the same cause must keep it always so, and must have kept it always so.

Upon these principles he proceeds to shew that, when the body is in motion, the velocity must be uniform, since any cause tending to diminish the velocity will be under the same conditions as that before considered which stops the motion. Or, again, that there is no reason why the increase or decrease of velocity should take place at any one point in its course rather than another. [Mech. c. i. prop. 8.]

Prof. Playfair has done little more than put the same kind of reasoning into an algebraical form in the proof he has given [Outlines, §. ii. art. 60.] which turns on supposing the velocity to change according to some function of the time, and upon our being unable to shew that the coefficients in that function are of one magnitude more than another. Upon this, it has been observed, that there would be no contradiction in supposing the velocity to decrease as the time increases.

Mr. Whewell's statement [Int. Dyn. 20.] of the first law is almost identical with those already cited. And his proof is, first, the body will move in a straight line, because if it move in any curve, some external cause must determine which way the curvature is to turn, which is contrary to supposition. Second, It must move with uniform velocity: because we find by experiment that we approximate towards this condition on removing interfering and retarding causes; and there is no internal principle which tends to diminish velocity.

He afterwards more explicitly says, "the thing to be proved is, that the velocity will not change without the action of extraneous force." (Int. to Dyn. 32.)

Also, "that a body has no power to alter its own velocity, is neither obvious nor certain, without recourse to experiment."

The same author, in his paper in the Camb. Trans. before quoted, gives the proofs more fully thus:

That part of the law which asserts that the *direction* will not change, is proved by reference to the non-existence of any cause or force which is necessary to produce it by Axiom i. and which does not exist, by hypothesis.

The velocity will be invariable, because in like manner it can only be affected by some cause dependent on space, or on time: and such causes would be forces, and therefore contrary to hypothesis. After stating this in detail, the author adds; "Thus it appears that in proving the first law, we obtain from our conception of cause the conviction that velocity will be uniform, except some cause produce a change in it; but that we are compelled to have recourse to experience, in order to learn that time alone is not a cause of change of velocity." (p. 7.)

Again, in the synoptical sketch, he states the "necessary" part of the first law to be, that "velocity does not change without a cause:" the "empirical" part, that "the time for which a body has already been in motion is not a cause of change of velocity."

Nature of the Experimental Evidence.

Now in regard to the *experimental* view of the matter, we may first remark, that the proofs alleged are in their nature necessarily imperfect. We have not, under any circumstances, any instance actually observed of absolutely rectilinear motion continued, even for a short time, with uniform velocity; much less continued *ad infinitum.* We only *infer*, that in proportion as certain interfering and retarding causes are diminished or removed, we approach towards these conditions, and by thus *abstracting* from the action of such causes, we *deduce by a process of reasoning* what the motion would be, or ought to be, rather than what it is, or is observed to be.

None of the motions actually occurring in nature are both rectilinear and uniform; as the motions of the planets, the fall of bodies by gravity, &c. Nor can we in these cases even perceive any application of the law, except *by assuming some principle* on which *to analyse* the observed motions into their component parts; which necessarily involves us in other considerations. The only cases on which the conclusion is ordinarily made to rest are such as the projection of bodies by impulse. A ball, e. g. is rolled upon a pavement: it continues at first to roll nearly in a straight line with nearly uniform velocity: it soon, however, begins to deviate from its path, and to lose its velocity: we are told this is due to the action of some irregularities in the surface; but how can we trace these compound effects without referring to the composition of forces? and if that principle depend on the laws of motion, we are arguing in a circle.

Such was, in fact, the process by which the enunciation of this law was first introduced. By such abstractions (it has been well expressed by Mr. Whewell) "they at last established by experiment a law which cannot be experimentally exemplified. The natural uniformity of motion was proved by examining all kinds of cases in which it was not uniform. Men culled the abstract rule out of the concrete experiment." (Hist. of Inductive Sciences, ii. 25.)

Thus our whole notion of uniform rectilinear motion is a purely abstract one; derived, no doubt, from contemplating the actual motions observed, and *in thought* analysing them into those elements which are the *simplest we can conceive:* these afterwards constitute the rudiments of our measures of motions and forces, and of all our dynamical investigations: but they are *not matter of observation,* nor have they any distinct existence in nature.

In the next place, let us look at the *reasonings* just referred to. When it is said that the thing to be proved is, "that velocity will not change without the action of some extraneous force," the whole question is brought without ambiguity into a narrow compass; and it is to my apprehension involved entirely in the definition of "force," and the distinction between extraneous and internal causes of motion. The former alone can, I conceive, have any place in dynamical reasoning; or, at least, the latter (if such should exist) would simply become subject of distinct investigation. Whenever we should observe motion increasing or diminishing, all we should have to do would be to investigate the law by which it might be regulated, to analyse the variable motions so observed into their component elements, and thus to trace the agency of the different separate causes of motion, or new elements of force, which might be successively introduced or withdrawn.

Definition of "Force."

The first requisite in any accurate examination of the subject appears to me to be the definition of the terms; a simple requisite, indeed, but one to which too little attention is commonly given. Now in the most general sense, the term "force," I suppose, would be defined by the writers referred to, as *that which produces, destroys, or alters motion*.

Thus then in Euler's reasoning the cause which is referred to as capable of retarding or stopping the motion, is simply *a force*; but *by hypothesis* we are abstracting from the action of all extraneous forces.

In D'Alembert's proof the whole reasoning turns upon the tacitly assumed definition of force. The argument is simply, that if the motion be not uniform, &c. then some new "cause producing motion" must act; which is contrary to hypothesis.

The same remark will apply to Prof. Playfair's investigation: if the velocity change at any instant, at that instant a new force acts; which is contrary to hypothesis.

There would be no impossibility in the supposition of a *decreasing* velocity; but there would at every instant of decrease be a new force subtracted.

If it be imagined that the mere circumstance of a body having *continued for some time* in motion, might produce the effect of either increasing or diminishing the velocity; or that we might say, in the one case, in the language of the poet,

> Mobilitate viget, viresque acquirit eundo;

or in the other, imagine with the Aristotelians, that though celestial and natural motions were perfect and incorruptible, and would therefore continue for ever; yet those which are violent or unnatural have a tendency to diminish and decay;—all this, it appears to me, would merely resolve itself into an examination of the action of *new forces* superinduced or subtracted to produce such changes.

Further; in the usual form of the law it is implied that the body is, in the first instance, already in uniform rectilinear motion.

Explicitly stated, then, it amounts simply to this, "that a body already in uniform rectilinear motion, not acted upon by any cause affecting its motion, will continue in uniform rectilinear motion:" or, in other words, its motion will continue unchanged until it is changed.

The moment, therefore, we introduce the definition of "force," and clearly state the original hypothesis, the law is not only true, but self-evident, and of that class of self-evident propositions called *truisms.*[*] In this sense, then, the so called first law of motion is really equivalent only to the *definition* of the term "force," or rather should be replaced by such a definition.

But here we must observe, that the term "force" has been as yet used and defined only in a very *general* sense: and there is *a more particular* meaning in which it appears necessary to consider it, and which ought to be included in our definition, before we can be prepared for the mathematical investigation of mechanical action.

In such investigations we have to analyse every observed or supposed case of motion into its *simplest* elements: and wherever any change of motion occurs, we have to resolve it into its component parts, and infer the amount of the new motion superinduced: the cause to which we refer it being regarded as a distinct force. The parts into which we thus analyse the effects of force must be such as are *capable of being subjected to definite measurement:* and such *alone* are *uniform rectilinear motions.*

Any such of the elementary parts into which the action of force in general is analysed, may be defined to be what is meant by the term "*a* force," or "*a single force.*" Some such distinction is obviously necessary, to prevent confusion; and the statement of it by a definition appears to me a requisite of primary importance to the whole course of reasoning.

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[*] This has been remarked, *en passant*, by the learned and acute author of the Life of Kepler, (ii. 45.) *Library of Useful Knowledge.*

It is further evident, in a mathematical point of view, that what is here said of motion in finite quantity, is to be applied in like manner when we come to introduce the method of limits.

But it is of no importance whether we can exhibit experimentally an instance of a force absolutely single. We indeed approximate to such by the removal of complicating causes: and this affords *illustration* of our definition.

We analyse observed cases of motion of a more complex kind into these simpler elements; by which means alone we can subject them to computation. We are led to recognise these as *the simplest* elements, in accordance with the truth that a straight line is the shortest between two points, and our most natural ideas of uniformity. On the other hand, from these simple elements, we proceed again by synthesis to investigate the results of different combinations.

The great problem of dynamics is, out of "single forces" to account for all or any cases of motion, in any way compounded or variable; accelerated, retarded, polygonal, or curvilinear. For the solution of this problem we must, in the first instance, adopt some such definition as this of "single forces;" and, in the next, examine into the principles on which we can thus proceed to the analysis or synthesis of proposed cases.

Mobility and Inertia.

We have thus far considered the meaning of the so called first law of motion, with special reference to the definition of terms. But, it may be asked, is there not really some *physical* first principle involved? or, (supposing the foregoing view of the subject admitted,) may not the proposition be yet understood, in another sense, as referring to some *real elementary property* of matter, for the proof of which we must, after all, have recourse to observation, and which must be presupposed in all dynamical applications, even of the definition?

In other words, (as indeed has been before remarked,) our definition might be very admissible, and the reasonings dependent on it abstractedly very satisfactory, but they might not necessarily apply to any state of things actually existing in nature: nor could we pretend to predict what results *must* take place in nature, except on the basis of some primary facts taken from observation, and adopted as first principles, in our dynamical system. We have, then, to inquire what *physical* suppositions are there which can be regarded as thus necessary, or more precisely what are the *fewest* and *simplest* facts of observation to which it is necessary to refer?

In the first place, there is obviously involved the *mobility of matter*: which is the experimental fact that the parts of the universe do not so forcibly cohere together, that some at least of those parts may not move with respect to each other. This experimental fact must certainly be known and admitted before we can advance one step in the application of dynamical reasoning to actual physical cases.

We also observe, that various agents or sorts of physical influence have the power of producing motion in bodies. The existence of such agents as gravitation, electricity, muscular power, magnetism, &c. are all primary facts of observation, but they are connected with dynamics only in respect to the *motions* produced: though we may analyse those resulting motions, and in some cases trace the mode of their production. Again, if we observe a body in motion, we may infer that it has received an impulse, from an analysis of its actual motion into simpler elements; but any inference as to the physical origin of such impulse is distinct from the application of dynamical reasoning. In other words, we should be particularly careful not to confound together the two senses of the term " force." In our definition we regard solely the effect or motion, which we take as the index of the amount and direction of a force. In this sense it must be kept entirely distinct from that of the particular *physical cause*, *origin*, or *acting power*, to which we refer, when we say the *force of gravity*, of *impact*, &c.

But, further, it may be asked, is not the *continuance* of motion *in general* a distinct physical truth, which may be described as a property of matter by the term *inertia?*

Now, in the first place, this property seems often stated as if it were only meant that matter is *passive*, and has no inherent power of motion: an assertion simply equivalent to the distinction between unorganised or dead matter, and that which is endued with the properties of vitality.

That matter can neither originate, alter, nor terminate its own motion, is certainly what we could not infer prior to observation; yet it appears to me that that fact is of very little importance to us in dynamical reasoning. I should merely say, if matter had an innate activity, that would not form a part of the problem we have to consider: or, if we imagine for a moment that observation should exhibit to us matter capable of setting itself in motion, or of changing its own velocity, it appears to me (as indeed has been already said) that we should only have a more complex sort of motion to analyse into its component parts, and to abstract so much as belonged to matter *considered as inert*, we should have, in short, a new species of force to examine, and to allow for its effects. *Matter, in so far as it is indifferent to rest or motion, or incapable of affecting its own motion, is the sole subject of contemplation in dynamics.*

In several parts of the writings of Galileo, and of some other philosophers of that period, we may trace a reference to what has been since called "inertia," though not mentioned under that name. But Des Cartes was perhaps the first who stated it formally as a distinct property of matter: he even viewed it not merely as a passive indifference to rest or motion, but as a sort of power exerted in order to preserve this indifference. He considered all motion as *naturally rectilinear*, and thence deduced the action of deflective forces to produce a curvilinear path. All this was intimately connected with the peculiar metaphysical principles of his system, into which it is not necessary here to enter.

With many writers of those ages the maintenance of such views may very probably be traced, in a great measure, to the necessity felt in the earlier stages of modern science for opposing the hitherto popular principles of the peripatetic schools. The inertia of matter, as an inherent and distinct property, was doubtless the more strenuously insisted on as contradicting the notion of the *tendency of motion to decay.* It was perhaps only running from one system of mysticism into another; and the formal maintenance of the laws of motion as axiomatic dogmas, may be traced to the lingering influence of the same scholastic spirit.

Some later writers have also dwelt much upon inertia as a peculiar property impressed upon matter, evinced in resisting the communication of motion, of which we should have had no conception prior to experience, and which, by a remarkable coincidence, is found from observation to be precisely proportional to gravity in bodies on the earth's surface, or to which our observation extends. To this, however, we shall recur hereafter. But more commonly, perhaps, the substantial truth intended to be affirmed amounts to this: that in the case of a physical force, or cause of motion, which can be applied to a body and then withdrawn, (and such a force is that of impact or pressure,) the motion which is produced in, or communicated to that body, *is retained by it*, (so long as no interfering cause opposes or changes it,) *even after the impelling body has ceased to be in contact.* This is perhaps regarded as a peculiar physical truth, or property of matter learnt only by experience. Here then, while we of course admit that this result *is proved* directly by experiment, we may yet make the *further inquiry*, Is this communication and retention of motion an *ultimate* experimental fact? or can it be analysed into a consequence of yet more simple principles?

To pursue this inquiry, we must briefly examine the first law, as stated by the French writers.

Inertia: First Law of the French Writers.

Laplace lays down, as the first law of motion, the inertia or tendency of matter to persevere in a state of rest or of motion; which he describes by saying, "Lorsqu'il est sollicité par une force quelconque, et ensuite abandonné à lui-même, il se meut constamment d'une manière uniforme dans la direction de cette force, s'il n'éprouve aucune résistance." (Mec. Cél. liv. i. c. ii.) This appears to me to be the true and perspicuous statement of the essential point. The whole question at issue is included in the notion of the body being *solicited by a force*, and THEN *abandoned* to itself. The only case to which I can conceive this description to apply in an intelligible sense, is that of the force of impact or pressure; where the material fact is, that the motion communicated is *retained, after the impelling body is removed.* This fact, taken generally, (without reference to the precise velocity or *direction* of the motion,) is what I should be disposed to consider the meaning of the term *inertia* as a physical principle. It remains to be considered whether we can *infer* this as a *consequence* from what we know of the communication of motion; if not, it must stand as an *ultimate fact.*

It is worthy of remark, that Laplace does not state the *rectilinear* character of motion as a part of the law of inertia, but deduces it separately as a *consequence* from that principle. With regard to the uniformity of the motion, he observes (ibid.): "La nature de la force motrice étant inconnue, il est impossible de savoir *a priori* si cette force doit se conserver sans cesse." He then refers to the usual experimental proofs which shew the tendency to this uniformity, in proportion as interfering causes are removed: the nature of which we have already considered. He also observes, that the most striking proof of inertia is, that the motions of the heavenly bodies have been preserved for so many ages without sensible alteration. This shews clearly that he uses the term *inertia* as referring to the *general* conservation of motion of any kind.

Communication of Motion.

The whole question appears to me to turn upon the reference here made to the *nature of moving forces*. Taken in all its generality, we certainly cannot reason upon the nature of such *forces*, so as to deduce any general conclusions. In such cases as gravitation, the condition supposed, viz. that the force is applied and then *withdrawn*, and the body *left to itself*, does not apply, since the force is continually acting, and the body never left to itself.

When, however, we limit our meaning to the case of *pressure*, then we can proceed on intelligible grounds to examine whether the result is not a *consequence* necessarily deducible from simpler principles.

We may first observe, that pressure may itself be produced by the action of the various physical forces or agents to which we have referred. Gravity, for instance, may produce pressure between two bodies; but our investigation of the nature and effects of the pressure is quite distinct from an inquiry into the nature of gravitation. The problem we have to consider is, the *result* of the action of one body upon another by pressure, without reference to the cause which may produce the pressure.

Thus the whole proposition resolves itself into the following points:

1. That as long as the pressure continues, or the pressing body is actually in contact, the motion will be communicated and continued.

2. That the motion will remain after the pressure ceases; which is the meaning of inertia, as before explained.

3. That the velocity retained after the separation of the two bodies will be that communicated by the impelling body while they were moving together.

By "pressure" we commonly understand that force which one body exerts upon another when they are either in contact for a shorter or longer time, or are inseparably connected together. And this force simply means that if one of the bodies pressing or connected be put in motion, it will produce motion in the other, supposing no obstacle in the way, and no other force acting. Now this is clearly a direct consequence, from the simple circumstance that the single points (in which we here suppose matter to be concentrated) are in the one case *impenetrable*, and in the other, by supposition, *inseparably connected*. Hence in the one case two such points *cannot* occupy the same point in space at the same time, and one must give way to the other; and in the other case one must carry the other along with it. That this is a necessary conclusion from the impenetrability of matter appears to me certain, so long as we admit the supposition that the body acted upon is *free to move* in any direction. If it were not moved when the other came in contact with it, that circumstance would be a proof that it was *not free* to move in any direction, and that some obstacle was opposed to its motion, or some other force acted on it; which would be contrary to the hypothesis we must make in all reasoning of this kind, and which accords with the primary property of matter, which we call its *mobility*. And in dynamics we only contemplate matter *in so far as it is* free to move in any, or at least in certain supposed directions. Thus upon these elementary ideas, which enter into our very notion of matter (derived of course from observation), viz. its impenetrability, its mobility, and the cohesion of its parts, *we infer by direct consequence the communication of motion.* The impenetrability of matter is nothing else than its solidity, the idea of which we acquire in the first instance from the resistance we find it opposes to the touch. Its cohesion is an equally elementary physical property, dependent apparently only on two particles being at a less distance than that at which resistance or repulsion exists, and where attraction prevails.

Inertia (*resumed*).

We are now prepared to resume the consideration of inertia.

If motion be communicated by pressure (i. e. as just explained), by the *contact* of a body in motion, and that moving body *be then stopped or withdrawn*, can we in this case *infer* the independent continuance of the motion communicated, after the impelling cause has ceased to act?

The grounds on which we can reason in this case appear to be these:

In order for one body to communicate motion to another in the same time, the former must have *a greater velocity*, by means of which alone it can produce an impulse on the latter.

When sufficient time has elapsed for the communication of motion, the two bodies (still in contact) move together with the same velocity: hence that which is behind can no longer affect the motion of the foremost. For it was by virtue of a greater velocity that it produced the effect in the first instance; but now that they have equal velocities, no further effect can be produced by their contact, unless the foremost should lose velocity: but for this some new force must act, which is contrary to hypothesis. Hence, since their being in contact does not affect the motion, that motion will continue the same though the hinder body be removed.

The term *inertia* is sometimes used to signify the resistance which matter opposes to the communication of motion, as well as to the conservation of motion once communicated. And this, like the momentum acquired and retained (when the velocity is the same), is in proportion to the mass or quantity of matter. This appears to follow from the consideration that if one material point evince such resistance, a number of combined points will each resist equally, and hence the whole effect will be proportional to the number of points, or to the mass. Now in bodies at the earth's surface we have to overcome the effect of gravity before we can communicate motion; but the effect of inertia, properly so called, must be regarded abstractedly from this.

Matter perfectly free from the effects of gravitation, and all other forces, would resist the communication of motion in proportion to its mass. But the question really is, what is meant by this *resistance.* A little consideration enables us to shew that this is no mystical occult property, but a consequence of the very simple principle, that to communicate motion from one particle of matter to another requires *time;* and more time in proportion to the number of intermediate particles; that is, motion is *longer* in being communicated to a larger mass. If we attempt to analyse the process by which a particle at rest acquires a finite velocity, it seems impossible to conceive the effect being produced instantaneously; we must infer that the particle has gone through all the intermediate degrees of velocity, from absolute rest up to that communicated. But if this should not be thought conclusive, we have only to allow *the fact* to stand as an ultimate experimental result, reducible to nothing simpler; *that motion is communicated in time.*

French Second Law.—Force proportional to Velocity.

That which is given as the second law of motion by the French mathematicians, viz. that "force is proportional to velocity," is established at length by Laplace. [Mec. Cel. liv. i. c. i. §. 5.] He proposes it in the following terms:

La force n'étant connue que par l'espace qu'elle fait décrire dans un temps déterminé il est naturel de prendre cet espace pour sa mesure. Mais cela suppose que plusieurs forces agissantes dans le même sens, feront parcourir un espace égal à la somme des espaces que chacune d'elles eût fait parcourir séparément, ou *ce qui revient au même que la force est proportionel à la vitesse.* C'est ce que nous ne pouvons pas savoir *a priori* vu notre ignorance sur *la nature de la force motrice*: il faut encore sur cet objet recourir à l'expérience; car tout ce qui n'est pas une suite nécessaire du peu de donnés que nous avons sur la nature des choses, n'est pour nous qu'un résultat de l'observation."

The author then proceeds to support this view of the matter by elaborate mathematical reasoning, ultimately referring to experimental proofs. He also adds a chapter in which he expressly investigates the results of supposing the velocity to be in any mathematical relation whatever to the force.

But this refined speculation may give us occasion to perceive yet more clearly the real nature of the whole question, as involved in the meaning of the term *force.* For what idea do we here frame of it, but that which is *defined by the particular function which it is supposed to be of the velocity.* It is this alone of which we have tangible conception, or which really comes within the grasp of our investigation. By "force" then, in these cases, is meant some cause which produces motion or velocity, and which is measured by the particular function of the velocity above expressed. It is then merely adopting *a different definition* of the term "force." If we assume a meaning restricted to a simple proportionality, then, it is said, we take that one among an infinity of possible suppositions, which is the actual law of nature. But it appears to me it would be more correct to say, in this case, we use the term *force* in the *simplest* sense we can give it, and the *ONLY ONE by virtue of which we can analyse dynamical effects into their measurable ELEMENTS or "single forces,"* agreeably to our primary definition.

Exactly the same in its principle as that of Laplace is the view of the subject taken by Poisson: who observes (Dynam. t. I. p. 278.) "La vitesse communiqué à la mobile par une force qui agit sur lui pendant un temps déterminé est une fonction du nombre qui représente l'intensité de cette force; le peu de donnés que nous avons sur la nature des forces ne nous permet pas de déterminer *a priori* la forme de cette fonction."

To this, similar remarks will apply. We may equally admit that the truth of the proposition rests ultimately *on the nature* of the moving force, or communication of motion, which is reducible to a matter of observation. But still our question is, may not this result of observation be further analysed? and what is the *simplest* experimental principle *up to which the nature of that communication of motion may be traced?* And (if the foregoing reasonings be admitted) this appears to be found in those most elementary physical facts, the impenetrability and mobility of matter.

We revert then to that essential point, the *definition* of the term *force.* If we understand "force" as simply that which is to be recognised in the velocity with which a body moves, the alleged law, "that force is proportional to velocity," is a mere statement of this in other terms; and is no more than *the definition of the measure of force*, for which we recur to the considerations already laid down. The effects of all dynamical action are reducible to observed cases of motion; and the *measures* of these, that is, of the rectilinear spaces uniformly traversed, compared with the time (whose lapse is uniform in accordance with our most primary ideas), are the *velocities* of the points affected by such motions: this being understood to apply either in actual ratio or in limit, according to circumstances.

And if the law be intended to express that the velocity with which a body continues to move uniformly (abstracting from retarding or accelerating causes) is the measure of the amount of force *originally impressed* as by impact, gravitation, &c., still the same remark applies.

Forces or impulses of different kinds and origins are comparable only by the effects which they produce of the same kind; that is, they are comparable only by means of the *motions produced* and resulting. And motion in a straight line with uniform velocity (as before), in actual value or in limit, is the *only measure* which we can adopt for comparing one dynamical effect with another.

In this sense then the thing asserted is, that *the velocity produced, and continued after the impelling body is withdrawn, is to be taken as the measure of the force of pressure.*

We know nothing of force except by our observations on motion, by which alone we measure and define it, or even (in a dynamical or mathematical sense) recognise its existence. In some particular cases, however, we may *connect* this observation of velocity with that of some other concomitant or antecedent circumstance, also subject to measurement: such, for instance, as the impelling action of another body, or, in more philosophical language, "pressure." Since we can in some cases estimate the amount of this action independently, and determine its relation to the velocity produced, we may term pressure a "force," meaning a species of physical agency; but we must be careful not to confound this meaning with that of "a single *force*," or dynamical element, according to our primary definition. Now supposing an inductive law of this kind established, asserting that the velocity is proportional to the pressure, it still does not follow but that the same result might not be derived by direct reasoning from yet simpler principles. That this is the case has been here shewn from very simple consideration of the primary physical property of matter —*its impenetrability and mobility.*

Moving Force and Pressure.

The proposition, that "when motion is produced by pressure, the moving force is proportional to the pressure," appears to me, like many of the other principles we have been examining, to resolve itself very much into definition of terms. We have already seen that pressure producing motion can be nothing else than one body, or particle, *in motion*, by material connexion, or by coming in contact with another at rest, communicating motion to it. When we speak of an effect *proportional* to the pressure, we must assume some *measure* of the pressure. This can be nothing else than the *velocity* of the moving body. Thus, for single physical points, the velocity communicated is necessarily proportional to that of the impelling point. And by moving force is meant the velocity multiplied by the number of points on the mass;—and, for the same mass, proportional to the velocity or pressure.

English second Law of Motion.

The second law of motion is given by Newton as supplementary to the first: "Mutationem motus proportionalem esse vi motrici impressæ et fieri secundum lineam rectam qua vis illa imprimitur." And from the instances immediately after adduced, of motions either conspiring or counteracting in the same line, or obliquely compounded, it would appear that the author contemplated this law in reference to the deviations from the conditions of the first law as produced by the introduction of a new force, chiefly with an eye to the deflecting forces which he was afterwards to analyse.

This enunciation has been considered defective in perspicuity.—The term "change of motion" does not perhaps carry with it sufficient evidence of its meaning: though when we consider the cases in which the law is exemplified, we perceive what is meant in the analysis which is made of the actual resulting motion arising from the combined action of two forces, and in the fact, that in such cases, the body is actually found as far advanced in *some direction parallel* to that of the impressed force, as it would have been *in the actual line* of that force had it acted alone; while at the same time it is as far advanced in *some direction parallel* to the original force or line of its original motion, as it would have been if the new force had not been introduced.

In Mr. Whewell's paper, in the Cambridge Transactions, the second law is stated thus: "When a force acts upon a body in motion, the effect is the same as that which the same force produces upon a body at rest." The author adds: "this law requires some explanation." Such explanation appears to me to turn wholly upon what is meant by "the effect" or the "change" of motion; and we are necessitated to seek some measure or estimate of this effect. With this view, after carefully drawing a distinction as to the use of the term force for the "accelerative effect" of force, the author refers to observation and experiment, to shew that the accelerative quantity of the same force is not affected by the velocity and direction of the motion of the body acted upon. This he exemplifies by the instance that a body falling vertically receives in any second of time an accession of velocity as great as that which it received in the first second, notwithstanding the velocity with which it is already moving.

But with respect to this, like other cases referred to, though undoubtedly capable of being shewn experimentally, my inquiry is, whether it is not *more* than a *mere* experimental fact, or is not susceptible of being yet further analysed: and whether (*in this particular case*) the effect is not a necessary consequence from the nature of gravity, as being near the earth's surface a constant force?

Again, in the short synoptical view which the author subsequently gives of the nature of the argument in each of the laws, we find the case of the second law thus stated:

That part of it which is a necessary truth is, that "the accelerating quantity of a force is measured by the acceleration produced." The empirical part, that "the velocity and direction of the motion which a body already possesses are not, either of them, causes which change the acceleration produced."

The first part I should regard as a definition, the second as undoubtedly proved by observation; but as a point on which again we have yet to inquire (as before) whether the experimental result may not be further traced up to some higher principle.

In the same author's systematic treatises the proofs are grounded mainly on the consideration that motion is *relative* to the part of space in which it takes place; and that the motion of a body is the same relatively to a given portion of space *to which it belongs*, whether that space be itself at rest or in motion. The proofs involving this principle are, however, referred ultimately to experimental facts as their basis. The experimental facts appealed to are chiefly the following:

First, that of a ball thrown across the deck of a ship, which strikes the opposite side at the same point, whether the ship be at rest or in motion. Under this example it appears to me there have been usually confounded together two cases which are in reality essentially distinct. First, when the ball is *rolled* across the deck; which is equivalent to the experiment of a ball rolling along a groove in a bar which is moved at the same time parallel to itself.

Secondly, the case when the ball is *thrown* across the deck through the air.

Thirdly, the fact that the vibrations of a pendulum are performed in precisely the same time whether the plane of vibration be coincident with that of the earth's motion or transverse to it.

Fourthly, that a ball dropped from the mast head falls on the deck exactly at the foot of the mast whether the ship be at rest or in motion.

Such are the experimental results usually appealed to: if any others be alleged, they will probably be found to be of a nature immediately referable to the same class.

In another passage Mr. Whewell has stated his view of the matter in more brief and distinct terms. [Mech. p. 230.] "According to the second law all motions are *compounded so as not to disturb each other;* each remaining, relatively, the same as if there were no other." Which he illustrates by pointing out what would be the state of the case if this law were not true, viz. that the above-mentioned experimental results would not take place.

Now in reference to the question whether these conclusions are simply inductive, or whether deducible from any higher principles, Mr. Whewell further observed (not. p. 231.), that in the case where a body is moved by a force acting *within* the space which is itself in motion, the truth of the law appears independently of experiment. As, for instance, in the case of the ball thrown across the deck of a ship, though not in that of the ball falling by the action of gravity from the mast head. That is, in the former case the ball is considered *necessarily* to partake in the motion of the ship; in the latter case *not necessarily*, though, from observation, it is *found* to do so.

In all these cases the *essential point to be made out is, that the one body belongs to or forms a part* *of the other*, and thus partakes in its motion: and it is an important question, not perhaps sufficiently adverted to, how and in what sense the one body can form a part of the other, or be thus shewn to partake in its motion.

In the first case, the ball is made to *roll upon an horizontal plane*, by being impelled in a given straight line. Now at every instant of its motion some one point of its surface is in momentary *mechanical contact* with a point of the horizontal plane which supports it. This contact is in fact due to the weight of the ball and the friction of the plane. By this material connexion, therefore, whatever motion is possessed by the point of the plane in contact is participated in by the point of the ball which touches it, and is for the instant a part of the same mass. By this material connexion, therefore, that point, and thence also every point in the ball, at that instant partakes in whatever motion be communicated to the plane. This will be the case with every point as it comes successively in contact with the plane. And thus, upon the whole, the ball throughout every part of its course under the influence of the impulse given it, will have precisely the same motion in that line, or will reach the same point in the plane in the same time as it would have done had the plane been at rest: and this will be equally the case in whatever direction the impulse be communicated.

Thus the *ultimate* experimental fact is that of the *participation in motion* by material contact. The sort of contact in the case supposed may be no more than that produced by friction, or it may be dependant on attraction, or some mechanical connexion: but in all cases of this sort we obviously must at last refer to a primary property of matter, viz. that its particles by this sort of union are capable of participating in all dynamical conditions for the communication of motion; which is in fact the property of solidity or cohesion.

If in the other cases no such analysis can be applied, they will then be reduced to bare experimental results resting simply upon inductive evidence.

But in the third case, I contend, the same considerations will apply as those to which we have referred in the preceding.

The points of suspension of the pendulum are in material connexion with the earth by resting on their supports: and thence, by material cohesion, every other part of the mass of the pendulum is in like manner in mechanical connexion with the earth. The motion belonging to the earth, and to the support of the pendulum, communicated to one part of its connected mass, is equally partaken in by all parts of that mass. Suppose, for illustration, it vibrates on a cylindrical edge: at every instant of a vibration some one point in the cylindrical surface is in *contact* (in the same sense as before) with the support: (which we will suppose a plane:) at that instant the point so in contact participates in the motion of the earth by virtue of its material connexion, as in the former case: and thus all points in the mass partake in the same motion; and the same holds good at every other instant. And this will be the case equally whether the plane of vibration be coincident with or transverse to that of the earth's rotation: or, in other words, is utterly independent of the particular direction in which the vibration takes place.

But let us imagine that delicate series of observations should detect any difference in the two positions; this would, to my apprehension, afford simply a proof, either that some new modification of gravity was discovered, or, more probably, that some other peculiar physical action (as e. g. magnetic or electric) was developed, which would form a distinct subject of investigation. Such a result would in no way affect my belief of the truth of the second law of motion.

This law then, as far as cases (1) and (3) are concerned, that is, as far as restricted to the conditions supposed in these cases, appears to be proved as a consequence from a more simple principle, viz. the general law or fact of the participation in the motion given to one body by any other which is connected with and actually forms a part of it.

Let us now proceed to the other instances: here there is somewhat more complexity: case (2), as I have objected, has been usually confounded together with case (1): but there appears to me an essential difference in the reasoning by which they are explained; and that the second is not deducible from the same principle as the first.

The essential point, as before observed, is to shew that the ball *partakes* in the ship's motion, or in some sense forms a part of it. In the former case (as we have seen) this is easily and conclusively shewn. In the present case it appears to me, that though the ball unquestionably *has* precisely the same motion which it would have if actually forming a part of the ship, as in the former instance, yet that it does so, is owing to more complex causes, and does not follow from that principle, nor as an *immediate and obvious* conclusion from any other.

For, to analyse the effect more closely, at the moment of quitting the hand the ball has two motions given to it; the one, that of impulse directly across the deck; the other, a communication (it is alleged) of the motion which the hand has as a part of the ship, in the direction of its course. Now this communication of motion is what we have to examine. And I contend, that it is not a mere *passive participation* in the motion of the ship, but an *actual impulse* given by, and, in the act of separation from the hand, in the direction of the ship's motion, and exactly equal to it. This will be seen at once, if we only for a moment imagine that the ball was set in motion, not by the hand, but by the pressure or impact of a plane, in a direction perpendicular to that plane, and at right angles to the direction of the ship's motion, which we will suppose precisely parallel to the plane: further, let the plane be conceived perfectly smooth and polished, so as to have not the smallest adhesion to the ball, or lateral effect upon it. In this case, though the surface were in motion in its own plane, *none of such motion would be communicated to the ball*, and it would consequently be left behind, or strike the opposite side of the ship at a point behind that at which it was aimed. In the actual case, a motion equal to the ship's motion is communicated as an impulse to the ball by the momentary lateral adhesion of the hand of the person throwing it. In this case, then, there is no proof of the law from the same principle as in case (1).

With regard to case (4), I conceive exactly the same principle will apply. The ball is supposed to be at rest in contact with a support at the mast head: when the ship moves, the motion is communicated to the ball by reason of its adhesion to the support occasioned by its pressure upon it. If the ball were devoid of weight, and the support perfectly polished, it would slip away from under it, and the ball be left suspended in the air. The pressure of the ball then on its support, and the roughness of that support, act exactly as an obstacle behind it would do; and in the instant of separation or withdrawing of the support, the obstacle acts with an impulsive force equal to the ship's motion, at the same instant that gravitation begins to act upon it also.

In neither of these cases then does the ball (after separation) *partake* in the motion of the ship *in the same sense* or owing to the *same cause* as in the other cases; and in neither of them can we apply the same simple principle to shew that the law is under these circumstances *necessarily* true.

In the second case the body has two simple impulsive forces acting on it at the same instant, and in the fourth it has one single impulsive force, and also the force of gravity, which acts in a different manner; that is, not in a single straight line, like an impulsive force, but equally *in* ALL *lines parallel to a given direction.*

The question then remains, is the proof in these cases to be set down merely as inductive? is it barely an experimental result, which we cannot analyse or trace up to any higher or more simple cause? I reply, that both these appear to me to be *cases of the composition of forces.* But if the parallelogram of forces be regarded as dependant for its proof on the assumption of the truth of this second law of motion, then of course it will be arguing in a circle to refer these results to it, and they will remain as bare experimental results.

If the composition of forces be regarded as proved *independently*, then these results will be referred to it, and appear distinct from the second law of motion.

It will be necessary then to look into the proofs of the parallelogram of forces; in reference to which it is well known different opinions have been held.

Composition of Forces.

The old proof, as given by Newton, and from him adopted by most English writers till very lately, has been censured as defective; and the French mathematicians have substituted others of a more abstruse description, but in which those defects are considered to be remedied.

Newton's proof is given as a corollary to the laws of motion, and is especially dependent on the second law: it turns essentially on the principle that a body, or point, which by the action of one force would in a given time describe one of the sides of the parallelogram, will, by virtue of that force, be always found at the same *distance* from the origin in that *direction*, measured either on the *same* line, *or on some line parallel to it,* whatever other force may act upon it. Or, in other words, a body is supposed to have an impulsive force acting on it, by virtue of which alone it would have a uniform motion in a straight line; but at the same time to be exposed to the influence of some cause or force, which, *at whatever point of its course it may be*, has an *equal* tendency to make it move, and in fact if at rest would make it move, in a given *direction*, that is, in any line *parallel* to a given line,—then the body obeying both influences at once, will move in a direction intermediate, which will be the diagonal of the parallelogram, of which the two given directions are the sides; and at the end of the same time, in which it would have described either of the sides separately, will have described the diagonal.

This sort of action of the force not solely in *one line*, but all lines parallel to a given line, implied in the expression that the force acting "in the direction of" (*secundum*) this side will in no way alter the velocity of the body's approach to the other side parallel to it, owing to the action of the second force: and for this the second law is appealed to. That the second law, therefore, must be understood in such a sense as to express this: or, in other words, it must express (under the terms "secundum lineam rectam" and "in eandem plagam") the idea of *a force acting in all lines parallel to a given line.*

Limited, then, to the conditions of this assumption, the proof is perfectly valid: the alleged defect (if it be one) does not exist *in the chain of reasoning*, which is complete and unbroken, but merely in the *limitation* imposed in the *hypothesis.* The proof, and consequently the conclusion, applies only in the case when *one*, at least, of the two forces acts not merely in one line, but in *all lines parallel* to it. For all cases, then, where this condition obtains, the composition of forces is strictly demonstrated upon the assumption of the second law of motion under the same limitation; which is no more than the assumption, that if a body be placed at successive distances from a given line which marks the direction of the force, it will at all those distances be equally acted upon by it. A case precisely similar to those before considered, in which this second law of motion is directly proved from a still simpler assumption.

The case (4) of those at first stated is one which, though (as was observed) it is not proved *directly* by the same considerations as (1) and (3), but is referable to the composition of forces, is yet such as falls under this simplest case of the composition of forces; and thus, though less directly, is yet with equal certainty, *proved from the same elementary assumption also:* the two forces acting being the *force* of gravity acting in all parallel lines, and the impulsive force acting in a single line communicated by the motion of the ship at the instant of withdrawing the support.

The only defect, then, in the old proof of the composition of forces, is merely that the conditions assumed are not perfectly general. It requires to be extended to cases where neither of the two forces is necessarily one, acting in all lines parallel to a given line, but where they may be any whatever. It is, I apprehend, in this that the difference of the modern proofs mainly consists. In these no assumption is made as to the particular *nature* of either force; they are represented by straight lines, and are considered as *impulsive* IN *those lines.* In proportion to this greater generality, the investigations are of course more prolix and abstruse.

Under this more general case falls the instance (2) at first mentioned: the ball is here acted upon at the same moment by two forces, each of which is a single impulsive force acting merely *in* one line, and not in parallel lines. This case, therefore, cannot be proved except by the composition of forces, and this in its most general form. Prior, therefore, to the introduction of these general proofs, it could be regarded only as an experimental fact: if they are admitted to be conducted without any assumption of the laws of motion, then this case will receive likewise its demonstration from the same first principle: not indeed directly or obviously, but through a long and somewhat abstruse train of deduction: yet (as I apprehend is generally allowed) in a manner strictly legitimate and satisfactory.

Now if we examine the proofs alluded to, we shall readily perceive that they *do not assume* the truth of the *second law of motion.*

Laplace's investigation (Mec. Cél. i. §. 1.) proceeds on the supposition of two forces, x, y, at right angles, whose resultant will be z, lying somewhere between them, and forming with x an angle θ.

Then taking small increments, θ remains the same; and dz will be in the ratio of dx and dy; or we shall have (ϕ indicating a certain function whose form remains to be determined,)

$$x = z \phi \theta \qquad y = z \phi \left(\frac{\pi}{2} - \theta\right)$$

He then considers x as compounded of x' in the direction of z, and x'' perpendicular to it: whence he deduces other values for x and y; and by these means obtains the *magnitude* of the resultant as being *equal to the diagonal.*

Then, to find its *direction*, he supposes again dx resolved into dx', dx''; and finds an expression for $d\theta$: whence, by integration, the form of the function ϕ is found to be,

$$x = z \cos \theta.$$

or the *diagonal* gives the *direction*, as well as *magnitude*, of the resultant.

Several other proofs which have been given do not differ from this *in principle*, though somewhat varied processes are employed in the details. Now, upon the closest examination of the nature of the reasoning, I think it will be allowed that the only assumptions here made are these:

First. That the single forces in question may be represented in quantity and direction by straight lines: this is simply the adoption of the definition which stands in the place of the first law of motion.

Secondly. That two such forces, placed at an angle, *will have a resultant somewhere between them.* This second assumption, then, is the only point which requires further examination. Now we may certainly, if we please, adopt it safely as an experimental fact. Suppose it thus regarded: still I contend that it is *an assumption very different from that of the second law of motion;* since there is here no supposition whatever respecting the *quantity* or *direction* of the motion remaining in the body; nor of the independence of one motion with regard to the other.

But to pursue the further analysis of this assumption:

Let a body be in uniform rectilinear motion, that is, in other words, be under the influence of a given force; and also be free to continue such motion indefinitely. Next, let some other force act upon it, which, if it acted alone, or on a body at rest, would produce a given motion. Thus much will, I conceive, be evident:

First. Unless the two forces are exactly equal and opposite, the motion cannot be wholly destroyed. Some motion will necessarily remain.

Secondly. It is a consequence, from the very definition of such forces, that the remaining or resulting motion is in general *dependent* upon *both forces jointly*, and upon them alone.

Thirdly. Let the two forces be represented by lines forming a certain angle: we in like manner infer that the body will not follow the direction of either of them: but,

Fourthly, The remaining or resulting motion must be *generally towards the region* to which they tend; that is, in some direction within the angle which they include.

Upon these simple considerations, it appears to me the assumption made by Laplace and others as the basis of their proofs is no more than a necessary consequence of the hypothesis of two forces impressed upon a point.

From what has been now advanced, it follows, that if we adopt the most general proof of the composition of forces, we in fact include under it that of all those cases which have been regarded as experimental results, evincing the truth of the second law of motion: and consequently we include the proof of that law. But though thus proved to be true, it is rendered unnecessary; and several of the instances referred to are seen to be no more than very simple cases of the composition of forces, such as can be readily established by particular methods, independently of the general proof.

If, for instance, we explicitly introduce the supposition that one at least of the two forces acts in all lines parallel to a given line, then it is easy to demonstrate the theorem of the composition of forces, either supposing the other to be also of the same kind, or a single impulsive force, after the method of the old proofs; and upon the obvious principle, that by the definition and assumption, the body placed at any point will be equally urged by the force in some line in the same direction.

But the more general case may be illustrated by considering the instance of a billiard ball at rest, and struck at the same instant by two other balls, whose directions form a given angle: the experimental fact is, that the first ball acquires a motion in neither of the two directions of the impinging balls, but in the diagonal of the parallelogram of which they are the sides.

Prof. Playfair has laid down a principle which he considers as equivalent to the second law of motion, viz. that "the motion communicated is always in the direction of the force impressed." It is given under the head of the first law; and he illustrates it by supposing a body acted upon by two forces at the same instant, which separately would cause it to describe two lines at right angles in the same time, and maintains that "the velocity of the body in one of these directions will not be changed by the force impelling it in the other." Of this he gives a demonstration *ad absurdum* by supposing an equal and opposite force to one of the rectangular forces, which ought therefore to retard the body, while it also accelerates it. (Outlines, i. §. 62.)

It appears to me that, under the ambiguity of the term "same direction," lurks the assumption of a force acting in all lines a *parallel* to a given line, as in other cases which are here noticed. This assumption restricts the generality of the investigation, and is precisely what renders necessary those more comprehensive, though of course more abstruse proofs, which the author censures. (§. 64.)

I will merely add one remark in reference to what was before said of Newton's proof of the composition of forces. The main application of this principle is in the first proposition on the equable description of areas. From the construction for the action of the centripetal force, by the help of finite rectilinear spaces, and the hypothesis of the rectilinear deflexion when that force acts "impulsu unico sed magno," it might appear that there was a departure made from the condition that one of the two must be a force acting in parallel lines.

But it will be admitted that the whole hypothesis is essentially that this "single impulse" is of such a nature, that the body goes on in the new path with the same uniform motion as before. Would it then (in order to reconcile this apparent difficulty) be doing too much violence to the sense of "impulsu unico," if we understand by it an uniform force acting in *parallel* lines, continued during the description of the small increment of the orbit? And is not this view of the incremental action of a central force that which is even most analogous and consistent with the corresponding view of the rectilinear increment of a curvilinear orbit? In the one case, we take the *straight* line, or side of the polygon, whose *limit* is the curvilinear orbit; in the other, we consider successive unequal forces in *parallel* lines, whose *limit* is the variable central, linear, force.

Upon the whole, then, it appears to me that those writers are fully borne out by the most exact scrutiny of philosophical reasoning, who, without referring at all to the second law of motion, proceed at once to the *general* proof of the parallelogram of forces, and upon that found all their subsequent reasonings.

Third Law of Motion.

Action and Reaction.

The third law of motion is stated by Newton thus: "Actioni contrariam semper et equalem esse reactionem: sive corporum duorum actiones in se mutuo semper esse equales et in partes contrarias dirigi."

The terms *action* and *reaction* are certainly of a somewhat vague and abstract nature; but, in the present case, the sense in which they are used is sufficiently apparent from the instances which the author immediately subjoins. These consist of the mechanical actions of bodies on one another by pressure, collision, &c. He refers particularly to the moving force of one body acting against another, and hints at the case of mutual gravitation.

Professor Robison (Elem. of Mechan. Phil. vol. i. p. 121.) seems to think that Newton regarded the third law as deducible *a priori*. He, however, himself conceives it referable to experimental evidence, but nevertheless "that it seems natural to imagine it a necessary law," and proposes conjecturally a theory of its principle or cause.

Maclaurin (Account of Newton's Discov. book ii. ch. 2. §. 28.) regards it as an inductive result.

Dugald Stewart (Philos. of Human Mind, vol. ii. note, p. 189.) observes, "My own idea is, that in the present state of our knowledge, it is at once more safe and more logical to consider it merely as an experimental truth, without venturing to decide positively on either side of the question."

Thus the proposition of the equality of action and reaction may be considered as intended to express a general inductive result, derived from the comparison of a great number of particular experimental facts; such as, that the moon attracts the earth in proportion to its mass as much as the earth attracts the moon: that the cart resists the horse as much as the horse draws the cart: the support presses a weight as much as the weight presses the support, &c. &c.

But I would suggest the question, whether this generalization be not more specious than solid? whether, under the vague terms *action* and *reaction*, (which are certainly extensive enough to include a multitude of the most dissimilar sorts of effect,) we are not here comprehending many particular species which have, in fact, no one common property which can entitle them to be considered as belonging to the same genus. Whether such sorts of action, for instance, as that of gravitation and of impact are by any means analogous in regard to what we call the reaction by which they are accompanied. Or, more precisely, whether the reaction in the one case be not simply an essential part of the observed experimental fact for which no further reason can be assigned ; whilst, in the other, it is a consequence necessarily and immediately deducible, from the nature of the action supposed.

In a word, I cannot better express my view of the matter than in the terms employed by Mr. Whewell in relation to another, but somewhat similar case: "It is a generalization, but a generalization established rather by enumeration of cases, than by any induction proceeding upon one distinct idea, like those generalizations of facts by which laws are primarily established. It rather serves verbally to conjoin laws previously known, than to exhibit a connexion in them : it is rather a help for the memory, than a proof for the reason." (Hist. of Ind. Sciences, ii. 41.)

In short, it appears to me that in this (as in many other similar propositions having a great appearance of simplicity and elementary generality) there is a very large infusion of verbal mysticism: certain specious and plausible expressions are employed to comprehend a most vague and uncertain meaning ; to class together objects which have nothing in common ; some of which are correct and important facts, and others merely unmeaning abstractions, adopted solely for the purpose of producing an apparent and nominal generalization of particular effects, having no real analogy or connexion.

The introduction of this law by Newton, and the illustration he gives of it, appear to me no more than instances of what we so often perceive in the style and manner of the Principia: a prudent regard to the prejudices the author was conscious he had to encounter in propounding such novel ideas as those involved in the doctrine of universal gravitation: and a wish to introduce them without unnecessarily raising hostility, or alarming the prepossessions of his readers, by as far as possible associating his new views with received ideas and modes of speech: hence the scholastic terms *action* and *reaction*, which would not be called in question, would well assimilate with the idea of the mutual action of gravitation. This, had it been stated nakedly and alone, as an attraction of every particle in the universe towards every other, would have borne so singular and startling an appearance, as would probably have caused its immediate rejection, as a doctrine absolutely chimerical and incomprehensible: but, under the metaphysical guise of action and reaction, it would be allowed to pass without much difficulty.

At any rate, I would further ask, (supposing the law admitted,) is there any consequence in dynamics built upon it? Is any demonstration essentially dependent on it? or in which, if it be in words introduced, it may not in truth and logic be dispensed with? Or indeed *can* a proposition of so very vague a nature be *of any use* in dynamical reasoning? From a general expression including under it two such entirely dissimilar truths as the *mutual* nature of gravitation, and the fact of a resistance opposed by matter to an impulse applied, could we possibly infer any other kind of reciprocal action? or deduce any valid or useful conclusions?

For example, action and reaction being equal in pressure and in gravitation, can we *infer* that they will be so in magnetism? If the law be universal, might we not conclude that it holds good in the attraction of the iron and the magnetic needle? or that the needle draws the iron ball as much in proportion to its mass as the iron draws the needle? Yet this is absolutely untrue.

By the term "reaction," it is implied that there is some opposing obstacle to be overcome, in order to communicate motion, and over that resistance the moving power must be preponderate. The nature of this resistance, indeed, in all actual cases in which we observe it, is a complex effect, resulting from a combination of causes, whose separate actions must be distinguished. In the case of gravitation, what is called the equality of the action and reaction is nothing more than the fact that gravitation is a *mutual* attraction: and this fact is a simple one, essentially forming a part of our notion of gravitation, which whole notion we derive from observation. Here, then, we can ascend to no higher principle.

But in other alleged cases of the mutual action of bodies, we can proceed by carefully abstracting those circumstances which render the effect complex, towards simpler elements.

Prof. Playfair gives the third law in Newton's words, and adds as equivalent, "in other words, the changes in the quantities of motion of the bodies which act, and of the bodies which are acted on, are equal and in opposite directions."

He distinguishes two cases: the first, when motion is communicated by impulse, (i. e. pressure), which he considers deducible from the nature of matter; the second, such cases as gravity, &c. where we only learn it as a result of experience.

This distinction appears to me very just. The "*nature of matter*," however, from which (I agree with the author) we can directly infer the communication of motion, requires to be more fully explained. This is what I have attempted to do in a previous part of this dissertation; and I have, I trust, sufficiently shewn it to consist of one or two simple elementary properties, which, as well as the property of gravitation, are reducible to nothing simpler, and may therefore be taken for ultimate inductive results.

But if these elementary properties are assumed as taught us by experience, it then appears that the communication of motion follows as a necessary consequence from them. This I conceive is the first consequence, and others flowing again from this make out the whole proposition.

Thus for the so called third law of motion, we are brought back to the principles before discussed and established.

Mr. Whewell states as the third law of motion the proposition, "When pressure communicates motion to a body, the moving force is as the pressure:" and alleges as the experimental proof of it such facts as that of two bodies suspended over a pulley, when the pressure of the heavier produces motion. The nature of this proposition, and the reasoning by which it is supported, have been before examined.

Conclusion.

In what has now been advanced, it will be seen that, upon the whole, I agree more nearly with the French writers than with the English in their exposition of the primary elements of our reasonings in dynamics: and that I differ from them only, or chiefly, in regard to the form of statement, and the definition of terms.

To recapitulate: I conceive it clearly impossible to refer to any other primary principles on such a subject than those of observation and induction. The question is, what are the *simplest* and *fewest* of such inductive truths, to which we can ultimately reduce all others? and they appear to be these:

(1.) The impenetrability of matter: (2.) Its capability of motion: (3.) The existence of various natural agents or powers capable of causing motion, as gravitation, elasticity, magnetism, muscular power, &c. &c.

To these are to be added the following, which seem to me *consequences* from the preceding; but which (if my reasoning be not thought satisfactory) will only have to be classed as ultimate experimental facts along with the foregoing: viz. (4.) The participation in motion by all the parts of the same connected mass.

(5.) The communication of motion from one body in motion to another with which it comes in contact.*

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* It will hardly be necessary to remind the reader, that in all which is here said of the communication of motion, &c. we are, of course, abstracting from all such effects as depend on the arrangement of the particles, such as would produce rotatory motion, &c.

(6.) The production of this effect not instantaneously, but *in time.*

(7.) The retention of such communicated motion, *after* the impelling body is removed.

With these I connect the definition of the term "force" in the more general sense, as that which causes and is indicated by *motion:* and in the more particular sense of *a* force, or a single force, the ultimate elements into which all compound forces are reducible, as being the simplest we can conceive, and the only cases susceptible of measurement; viz. those which are measured by motion with uniform velocity in a straight line. We speak of the force of gravity, of impact, &c. in the former of these senses.

With the aid of these principles alone, I conceive we can proceed at once to the demonstration of the parallelogram of forces in its most general form; as the French writers have done. And here again, to guard against all possible objection, we may (if thought desirable) leave one more point as an ultimate experimental fact; viz. (8.) that two single forces, supposed applied at one point, will have *one* resultant; though it appears to me a consequence from the preceding.

NOTES AND ILLUSTRATIONS.

I. (p. 14.)

DUGALD STEWART, after observing that the first principles of *all* the sciences are intimately connected with the philosophy of the human mind, and such as call for the utmost logical precision for their satisfactory enunciation, remarks; "It is in stating such principles, accordingly, that elementary writers are chiefly apt to fail. How unsatisfactory, for example, are the introductory chapters in most systems of natural philosophy; not in consequence of any defect of physical or of mathematical knowledge in their authors, but in consequence of a want of attention to the laws of human thought, and to the general rules of just reasoning." (Philos. of Human Mind, vol. i. p. 58.)

In another place, speaking of Newton, the same writer observes; "But the language of this great man, when he happens to touch upon logical questions, must not always be too literally interpreted. It must be qualified and limited so as to accord with the exemplification which he himself has given of his general rules." (Philos. of Human Mind, vol. ii. p. 426.) It is no disparagement to Newton's philosophical preeminence to allow the justice of this remark, which it may be peculiarly proper to bear in mind in a discussion like the present, chiefly relating to points which it formed no part of his design to examine minutely.

Some perhaps may think that the topics discussed in this Essay belong rather to the earliest stage of the philosophical investigation of motion; that at the present day it is idle and useless to conduct our inquiries over paths long ago so completely beaten; and that in the existing state of science we are not to expect any new light can be shed upon points of so elementary a character.

Yet in the kindred subject of hydrostatics a case precisely in point has occurred within these few years. The fundamental property of the equal pressure of fluids had been regarded as an elementary fact of observation, until Mr. Airy, in his lectures at Cambridge, shewed that it might be analysed up to a still simpler principle, viz. to the general fact that "the division of a perfect fluid may be effected without the application of sensible force:" from which it immediately follows, that the state of equilibrium or motion of a fluid mass is not altered by mere separation of its parts by an indefinitely thin partition. And the principle thus established has been adopted by Prof. Miller, in his "Elements of Hydrostatics," Camb. 1831. See Prof. Challis's masterly Report on the present state of Hydrostatical and Hydrodynamical Science, Reports of British Association, vol. ii. p. 133.

The reader will hardly need to be referred to Mr. Whewell's History of the Inductive Sciences for a complete general view of the progress of philosophical opinions on the subject of the primary principles of motion. (See vol. ii. ch. 1.) In addition to the authorities there referred to, the following extracts may assist in conveying some more particular illustrations of the subject.

The views of Des Cartes, and the three laws which he proposed, will be found sufficiently explained in the following passages from his Principia Philosophiæ:

"Atque ex hac eadem immobilitate Dei, regulæ quædam sive leges naturæ cognosci possunt, quæ sunt causæ secundariæ ac particulares diversorum motuum, quos in singulis corporibus advertimus. Harum prima est unamquemque rem, quatenus est simplex et indivisa manere quantum in se est in eodem semper statu, nec unquam mutari, nisi a causis externis. Ita si pars aliqua materiæ sit quadrata, facile nobis persuademus illam perpetuo mansuram esse quadratam, nisi quod aliunde adveniat quod ejus figuram mutet; si quiescat non credimus illam unquam incepturam moveri, nisi ab aliqua causa ad id impellatur: Nec ulla major ratio est, si moveatur, cur putemus ipsam unquam sua sponte, et a nullo alio impeditam motum illum suum esse intermissuram." . . . . . . (Pars ii. §. 37.)

§. 39. "Altera lex naturæ est, unamquamque partem materiæ seorsim spectatam non tendere unquam, ut secundum ullas lineas obliquas pergat moveri, sed tantummodo secundum rectas." . . . . . . "Causa hujus regulæ eadem est quæ præcedentis, nempe immutabilitas et simplicitas operationis per quam Deus motum in materia conservat." . . . .

§. 40. "Tertia lex naturæ hæc est. Ubi corpus quod movetur alteri occurrit, si minorem habeat vim ad pergendum secundum lineam rectam, quam hoc alterum ad ei resistendum, tunc deflectitur in aliam partem, et motum suum retinendo solam motus determinationem amittit: si vero habeat majorem, tunc alterum corpus secum movet ac quantum ei dat de suo motu, tantumdem perdit." . . . . "Atque omnes causæ particulares mutationum quæ corporibus accidunt, in hac tertia lege continentur." . . . .

§. 41. "Demonstratur autem prior pars hujus legis ex eo quod differentia sit inter motum in se spectatum et ipsius determinationem versus certam partem." . . . . .

§. 42. "Demonstratur etiam pars altera ex immutabilitate operationis Dei, mundum eadem actione qua olim creavit, continuo jam conservantis." . . . .

In the Philos. Trans. 1669, N°. 43, p. 864. Dr. Wallis published his "summary account of the general laws of motion," &c. He here lays down the general principles of proportionality between the force and the velocity; of which he gives demonstration on abstract principles: he considers also the case of impact. But his views are more fully developed in his treatise entitled, "Mechanica, sive de Motu:" (1670.) It commences with definitions of motion, momentum, &c. chiefly verbal and etymological: the first propositions are lemmas, wholly geometrical, referring to the doctrine of ratios.

The tenth proposition is the first referring to motion: this and the eleventh relate to the resulting action of a certain "momentum" and "impedimentum," according to the excess of either: the last clause is, "si æquipollent, neque ponitur motus neque tollitur: adeoque quæ prius erat vel quies vel motus, perseverat." To which he adds the following scholium: (p. 19.)

"Postremam hujus propositionis partem, nempe inceptum motum (nisi obstaculum ponatur) suapte sponte (sine continuo motore) non minus quam jam existentem quietem (nisi accedat motor) perseverare, Galilæus, Cartesius, Gassendus, aliique videntur postulare: atque hinc non levis momenti multa inferunt: Qui autem demonstret non memini me vidisse quempiam. Eratque hoc nobis, in sequentibus asserendum, ubi de motuum acceleratione dicetur: tam propere tamen, in ipso statim vestibulo, asserendo abstinuissem, nisi consequentiæ necessitate viderem me coactum jam statim affirmare; non parvi postea momenti futurum."

In the Histoire de l'Académie Royale des Sciences à Paris, tom. I. (1674.) p. 182. we have an account of researches under the head of "Mécanique," carried on by M. Mariotte. These relate principally to the collision of bodies; but they are introduced by some general considerations, in which he observes, "Il y a pour leur communication (des mouvements) des règles très simples et qui regnent partout; mais dans presque tous les effets que nous voyons elles sont si envellopées et si étouffées sous la multitude des différents circonstances, qu'il est très difficile de les démêler, et de parvenir à les voir dans leur simplicité naturelle. Le secret est d'écarter d'abord le plus de circonstances qu'il est possible, et de n'envisager que les cas où il en entre le moins." No further statement, however, is given of any general principles corresponding to the laws of motion.

In the same collection, (1706, p. 442.) is a memoire, "Des Loix du Mouvement par M. Carré;" in which the author proposes to supply the deficiency of his predecessors by demonstrating a general principle: but this refers chiefly to the collision of elastic bodies, and bears little upon the philosophical discussion of the real first elements of the science.

In 1674 Hooke published his "Attempt to prove the

Motion of the Earth from Observation;" in the conclusion of which he proposes some idea of a system of the world grounded upon three "*suppositions*," two of which refer to the attractions of the planets; and the other is, "that all bodies whatsoever that are put into a direct and simple motion, will so continue to move forwards in a straight line till they are, by some other effectual powers, deflected and sent into a motion describing a circle, ellipsis, or some other more compounded curve line."

But the same author, in another place, has entered more at large on this point. In his posthumous works (p. 355.) we find a demonstration of the spheroidal figure of the earth; "In order to which demonstration," he says, "I must premise this principle of motion, that every body that hath received or is moved with any degree of motion, if it receives no other motion from any other body whatsoever, will constantly persevere or continue moved with the same velocity in the straight line of its tendency infinitely produced. The reason of which is this, that no reason can be assigned why its motion should cease where there can be no impediment. Nor is there any reason why it should deflect to any side out of its direct way, since from the supposition there can be no new motion added to it from any other body.

"Now this being a principle, will not admit of any other demonstration than that of induction from particular observations in natural motions, by which all such principles are made; for whosoever shall strictly and accurately examine and analyse all local motions, will find hundreds of instances that, after a due analysis is made, do sufficiently evidence the universality and certainty of this principle in all local motions."

Varignon, in his paper entitled, "Règles du Mouvement en général," (Mém. Acad. Paris. tom. x. p. 225, 1692.) speaking of the labours of preceding philosophers, refers to Galileo as having first distinctly laid down the laws of motion, on which all mechanical reasoning must rest: and, after him, notices Des Cartes, le Pere Fabri, Borelli, and others, as having carried on the same researches. These writers, however, are represented as not ascending to sufficiently high and general principles; and Wallis is praised for commencing his treatise by an investigation of motion in general. It is observed, however, that the method was such as to lead him to but few general laws; and that these *he only proved by induction*, and not in a manner properly universal. The investigations of M. Varignon were therefore undertaken with the view of remedying these defects; and a series of propositions as to the relations of force, mass, and velocity, are there laid down, chiefly dependent on one or two first assumed. The fundamental one seems to be, that in every kind of motion, under every conceivable variety of circumstances, "the sum of the forces which produce the motion, at every instant of its continuance, is always proportional to the sum of the paths, or the lines traversed by all the points of the body moved."

From this, several other propositions are made to follow relative to momentum, &c.; but none of these appear to bear any resemblance to what are usually considered the laws of motion, nor are they accompanied by any proofs, either inductive or *a priori*.

Huyghens commences his investigation by laying down certain "hypotheses," of which the first is, "corpus quodlibet semel motum si nihil obstet, pergere moveri eadem perpetuo celeritate et secundum lineam rectam."

The second is to the effect, that two hard bodies in collision will each rebound with their respective velocities.

The third asserts the *relative* nature of motion.

The fourth, inserted afterwards among the theorems, (p. 373.) "Si corpus majus minori quiescenti occurrat, aliquem ei motum dare ac proinde de suo aliquid amittere."

The fifth (p. 374.), "Corporibus duobus duris sibi mutuo occurrentibus, si post impulsum, contingat alteri eorum omnem quem habebat motum conservari, etiam alterius motui nihil decedere neque adjici." (De motu corporum, &c. Opusc. Post. Lugd. Bat. 1703, p. 369.)

Euler, in his essay D'une Démonstration Métaphysique,

&c. (Mém. Acad. Berlin. 1751, p. 246.), lays down the following considerations as to the nature and definition of force :

" On nomme *force* tout ce qui est capable de changer l'état des corps, tant de leur mouvement que du repos : car puisque tout corps de soi-même demeure toujours dans le même état, soit de repos ou de mouvement, s'il y arrive quelque changement, la cause subsiste nécessairement hors du corps : et c'est cette cause quelle qu'elle soit, qui est nommée *force*. Dans chaque force il y a deux choses à considérer, la quantité et la direction : par la quantité on comprend combien une force est plus grande ou plus petite qu'une autre, et la direction nous donne à connoitre en quel sens chaque force agit sur les corps pour en troubler l'état."

He then gives illustrative examples to explain our ideas of the direction and quantity of force. With respect to the former, he observes ; " Ainsi la direction est toujours la ligne droite sur laquelle la force *tend* à transporter le corps: et cette *tendence* suffit à déterminer la direction, puisqu'il n'est pas ici question du mouvement actuel, qui sera imprimé au corps par la force, ce qui demanderoit sans doute une recherche plus profonde, et qui ne seroit plus du ressort de la métaphysique."

His illustration of the *quantity* of force is conveyed by means of the idea of a body acted on by a system of strings (filets) connecting two parallel bars, and having a contractive power.

He afterwards adds ; " Ayant donné cette idée de l'action de chaque force, on en tirera aisément ce principe général :

" Que toute force agit autant qu'elle peut. Et dès qu'on aura compris le sens de cette proposition, on ne pourra refuser de l'admettre comme un axiome. Car puisque l'action d'une force consiste dans la contraction des filets dont nous concevons la force composée, ces filets ne cesseront pas de se contracter tant qu'ils ne rencontrent pas un obstacle invincible, qui s'oppose à leur contraction ultérieure : donc ces filets et partant aussi la force qui en est composée agira autant qu'elle peut, ou que les circonstances lui permettront d'agir."

II. (p. 16.)

Euler's proof of the first law of motion in his "Mechanica" depends, in the first instance, on the following corollaries :

§. 62. Cor. 3. Simili modo, quo evicimus corpus semel quiescens perpetuo quiescere debere, nisi a causa externa afficiatur, potest ostendi, corpus, quod nunc quiescit absolute, antehac semper quoque quievisse, siquidem sibi ipsi fuerit relictum. Uti enim nulla est ratio, quare potius ex hac, quam illa plaga in eum, quo nunc stat, locum pervenerit, ita concludendum est etiam in eo loco antea semper constitisse.

Cor. 4. Corpus igitur, quod semel quiescit si ulla causa externa in id neque agat, neque egerit, id non solum in posterum quiescit semper, sed etiam ante perpetuo quievisse statuendum est.

Cor. 5. Sequitur ex hoc, corpus semel absolute motum in quietem pervenire nunquam posse sibi relictum. Nam si tandem quiesceret, idem oporteret antea quoque semper quievisse, quod est contra hypothesin.

The main part of the proof then follows :

Prop. 8. Si enim corpus motum celeritatem non conservaret semper eandem, tum vel augeri deberet vel diminui ejus celeritas. Hoc autem casu ad quietem inclinaret, quod quia nunquam consequi potest (62.) accidere nequit. Illo casu vero ex quiete provenisse censendum esset, quod æque foret absurdum. Præterea si hoc corpus in spatio infinito et vacuo positum concipiatur, ejusque via, qua est ingressum et ingredietur, consideretur ; nulla est ratio, quare potius in hoc majorem minoremve habeat celeritatem, quam in illo loco quo circa perpetuo eadem moveri debebit celeritate. Q.E.D.

Prof. Playfair's proof (Outlines, &c. §. ii. art. 60.) is in substance as follows :

If the velocity (v) change, it must change according to some function of the time (t) ; such as,

$$v = c + at^m + bt^n + \&c.$$

Now there is no condition involved in the nature of the case by which the coefficients a, b, &c. can be determined to be of any one magnitude rather than of any other: hence there cannot be such coefficients in $a=0$, $b=0$, &c. and therefore $v=c$, or the velocity, is uniform.

In merely a mathematical point of view, this reasoning seems to me to stand on very questionable ground.

It has also been remarked, that in this way we might as well suppose the velocity to *decrease* in a geometrical ratio as the time increases, or that the law might be $v=cm^{-t}$: a supposition on which I have above commented.

III. (p. 24.)

The nature of force in general is discussed by Sir J. Herschel in his discourse on the Study of Natural Philosophy, (Cab. Cyclop.) where (p. 86.) he observes, that we have a sensation within ourselves whenever we exert muscular action, to which we apply the name *force*: that this idea is perfect, even when unaccompanied with any production of motion; as in pressing our two hands violently together: and that it is only by observation that we connect together *this* idea of force with that of motion.

The idea which we thus form of what we term *muscular force* is doubtless that of *one species* of physical force, using the term in the sense of a *power* or *origin* of active force: and we certainly could not *a priori* connect this idea with that of motion, but finding it capable of producing such an effect, we class it as one among various species of physical force, as gravitation, electricity, pressure, &c. These may be recognized in various physical effects which they produce; but they become subject of dynamical investigation, solely in respect of the motions to which they give rise.

Again, the same author observes, (p. 222.) "Matter, or that, whatever it be, of which all the objects in nature which manifest themselves directly to our senses consist, presents us with two general qualities, which at first sight appear to stand in contradiction to each other, activity and inertness.

Its activity is proved by its power of spontaneously setting other matter in motion, and of itself obeying their mutual impulse, and moving under the influence of its own and other force: inertness, in refusing to move, unless obliged to do so by a force impressed externally, or mutually exerted between itself and other matter, and by persisting in its state of motion or rest, unless disturbed by some external cause. Yet, in reality, this contradiction is only apparent, force being the cause and motion the effect produced by it on matter. To say that matter is inert, or has *inertia*, as it is termed, is only to say that the cause is expended in producing its effect; and that the same cause cannot (without renewal) produce double or triple its own proper effect."

I will merely add, that in this passage I presume the "activity" of matter is to be understood as referring to various agencies in nature capable of producing motion: and we must still bear in mind the distinction before adopted as to the twofold sense of the term *force*.

Poisson (Mec. Anal. i. 178.) calls the law of inertia an "hypothesis;" but it is clear that by this term he means an hypothesis *prior to mathematical reasoning*; that is, an assumption from the results of experience: since he considers it a marked difference between dynamics and statics, that the latter "does not depend upon any *hypothesis*."

IV. (p. 27.)

It may illustrate our ideas of inertia, to observe the nature of some speculations which writers on physical astronomy occasionally pursue respecting it. Thus, in considering the different masses, and the consequent differences in the intensities of gravity, which must subsist at the surfaces of the planets, it is sometimes proposed to determine also the different ratios which on their several globes the *inertiæ* of bodies must bear to their *weights*. *This* may appear to carry with it an appearance of referring to some occult and mysterious property; but it simply amounts to this: If the same mass of matter were transported from the earth to a planet whose mass is different from that of the earth, its weight would of course be altered in the ratio of the altered gravitation. But (understanding by *inertia* the conservation of, or resistance opposed to, impulse) the same impulsive force impressed upon it (or indeed any force other than that of gravity) would produce the same motion, or the body would offer the same resistance: hence in this sense the ratio of the inertia and gravitation would be different.

At the *same* planet the inertiæ of different bodies are proportional to their masses or gravities. The resistance or communication of motion by impulse, &c. is proportional to the quantity of matter: this follows *necessarily as a consequence*, from the nature of the communication of motion. The gravity is also proportional to the mass, not from any such necessary influence, but simply as an observed fact, which indeed constitutes an essential part of our notion of gravitation.

Some writers have described inertia as an *active* "power of maintenance;" and would appear to understand by the term a sort of capacity for receiving and retaining the effect of gravitation, such that we may conceive a variation in the effect produced to be due either to an increase in the absolute intensity of the supposed power or force called *gravity*, or equally to a diminution in the susceptibility of the same mass of matter to its influence. Thus the common problem of the change in the *force* necessary to convert an elliptic into a parabolic orbit, is sometimes propounded as referring to a corresponding change in *inertia*. A planet, moving in an elliptic orbit, it is said, in order to have that orbit changed into a parabola, or to become a comet, must have "*either* the force of gravity diminished (in Newton's notation) in the ratio $2AC : PH$, *or* the inertia increased in the same proportion."

Prof. Playfair, speaking of the mutual connexion and reaction of astronomical and mechanical discoveries, observes: "The inertia of matter, or the tendency of a body when put in motion to preserve the quantity and direction of that motion after the cause which impressed it has ceased to act, is a principle which might still have been unknown, if it had not been forced upon us by the discovery of the motion of the earth." (Historical View of Science, Works, vol. ii. p. 145.)

I should state the case simply thus: We are utterly ignorant of the physical cause or power which originally impressed motion on the earth; but we know by comparison of observations that it continues unchanged. If it were found to alter, the only inference would be, that some other cause must act to produce the change; which would become a subject of distinct investigation.

Dr. Brown contends ("On Cause and Effect," p. 235.) that there is no necessary truth in the supposition of *inertia*, and that continued rest or continued motion are only particular cases of the general uniformity of the continuance of like effects under like circumstances. He conceives that we should form no notion of such a continuance of rest or motion in matter prior to experience.

In denying the application of the principle of "sufficient reason" to the proof of the first law of motion, he however appears in one instance to have mistaken the conditions of the case; for he contends that we cannot determine *a priori* that some totally different sort of motion might not take place, as e. g. the body might suddenly divide into parts, which might fly off in all directions, like an explosion. This, however, is manifestly inapplicable, since the conditions of the problem restrict us to considering the motion of the whole body as one mass.

He examines at length, and with great acuteness, the cases of inertia, both as to rest and as to motion, and appears to me satisfactorily to refute the application of the principle of "sufficient reason" to the assertion of what must be the result in any, even the simplest cases of physical action. He indeed contends, that such determination can be made only on the ground of experience. Here I cannot agree with him: I conceive that there are certain elements of physical truth at which we must stop as ultimate inductive facts; but the question in regard to the laws of motion and mechanical action is, to what extent we may carry our analysis before we arrive at these ultimate principles. The same remark will apply to his Observations of the Communication of Motion, p. 289.

His general notion of force, as being "a word expressive of antecedence to change," (p. 337.) accords with what I have observed above.

Dr. Brown proceeds to shew the futility of the principle of "sufficient reason" as applied to prove that the velocity will be uniform, contending that to assert there is no cause which can produce increase or decrease of velocity can be justified only by experience. (p. 268.) He fully refutes, on this principle, Euler's demonstration (p. 271); as also the application of the same principle to the remaining part of the proposition, viz. that the motion will be rectilinear. (p. 284.)

After remarking, in the first place, "that in the determination of the question" (relative to the necessary truth of the laws of motion) "all abstract reasoning of pure mathematics is by its very nature excluded," and that though properly applied to measure, velocity, force, momentum, &c. they cannot determine anything with respect to the existence of these physical principles in the first instance; he afterwards proceeds to state more fully his view of the principles on which all reasoning on these points must depend (p. 238), which he afterwards repeats, (p. 251.)

There is doubtless much truth in these remarks; but the question which I am concerned in examining is little affected by them. My inquiry is directed to this, What are *the simplest* principles to which we can ascend in our investigation? These, I contend, are something simpler than the usually recognised laws of motion. They involve, unquestionably, certain truths relating to the nature of matter, and learnt by induction, but those truths are of the most elementary simplicity;—the mobility and impenetrability of matter; the existence of gravity, &c.

The principle of sufficient reason was used by Archimedes in his demonstration of some propositions in mechanics, but was first stated as a general principle by Leibnitz. It was characterized by one of his opponents as "only a very ingenious method of setting up our ignorance as an instrument to ascertain, and a standard to judge of truth."

For an account of Leibnitz's first idea of its application to mechanics, and some remarks on its supposed axiomatic nature, see D. Stewart Philos. of Human Mind, vol. ii. c. 2. note p. 195.

We have also seen that this principle, or what is equivalent to it, was adopted by Hooke.

Professor Playfair considers it to be on this principle that time is divided into equal portions, since the vibrations of the pendulum, &c. are always determined by precisely the same circumstances. (Outlines, i. 20.)

By Dr. Brown, however, it is considered to be absolutely fallacious, both in itself intrinsically, as well as in its application to the establishment of any of the elements of physical truth. (p. 243.)

In many cases, I believe, what is called the application of this principle is really reducible to a mere question of definition of terms.

In others it seems equivalent to throwing the "onus probandi" on the most improbable side.

V. (p. 28).

The subject of the communication of motion by impulse (or pressure) has given rise to very abstruse disquisitions among metaphysical writers. Some have contended that even if no experiments had ever been made, yet the result might have been predicted *a priori*. (See Dr. J. Stewart's Answer to Lord Kaimes's Essay on Motion.) Others, on the contrary, have found so great difficulty in conceiving the *modus operandi*, as to pronounce the effects altogether inexplicable. They have even considered the preservation of motion, after the impelling body is removed, to be due to nothing short of the agency of *mind*, with which they consider matter to be animated.

Dugald Stewart observes, (Philos. of Mind, i. p. 77.) "Although, however, it is obvious, on a moment's consideration, that we are as ignorant of the connection between impulse and motion, as of the connection between fire and any of the effects we see it produce, philosophers, in every age, seem to have considered the production of motion by impulse, as almost the only physical fact which stood in need of no explanation. When we see one body attract another at a distance, our curiosity is roused, and we inquire how the connection is carried on between them; but when we see a body begin to move in consequence of an impulse which another has given it, we inquire no further: on the contrary, we think a fact sufficiently accounted for if it can be shewn to be a case of impulse." This is precisely the point at which I would take up the subject, and explain this communication of motion by the impenetrability of matter: and this accords precisely with the view of the subject taken by D'Alembert; who observes, "Les vrais principes d'où l'on doit partir dans chaque science sont des faits simples et reconnus, qui n'en supposent point d'autres, et qu'on ne puisse par conséquent ni expliquer ni contester; …… en mécanique, l'impénétrabilité des corps, source de leur action mutuelle." [Elém. de Phil. p. 26.]

The ideas commonly entertained of the force of impact, have been clearly exposed by Mr. Whewell, in a paper before referred to. He studiously guards against the fallacy (as he shews it to be) in some of the French writers, of first considering the instantaneous action of a force without respect to time, reasoning upon it as a *statical* force, and thence transferring the conclusion to *dynamical* force, or that which is measured by motion. He argues against the notion of "supposing all velocities in bodies to be produced by forces acting instantaneously, that is, acting at a single indivisible and uncontinued moment of time. For the fact really is, that there are no such forces in nature, and that forces of impulse and impact, which appear at first sight to operate in this manner, seem to do so only in consequence of a vague and imperfect mode of conceiving them, and do in reality produce their effects by a continued pressure acting for a short but finite portion of time. And this is the case with all forces which ever come under our observation; insomuch that we cannot in strictness suppose any such process as a body at rest acquiring *at once* a finite velocity, without going through the intermediate degrees of velocity." [Edinburgh Journal of Science, No. XV. 30.]

(VI. p. 33.)

Profound and penetrating as was the genius of Laplace when once fairly engaged among the actual relations and combinations of quantity, in disentangling the most intricately involved, and bringing together the most remotely separated elements of mathematical speculation, it is no disparagement to his preeminent merits to say that in other departments of inquiry he might not evince the same elevated powers. And in the very humble task of accurate inquiry into the niceties of elementary distinctions, in the precise criticism of the ultimate elements of reasoning which belongs to a period of the investigation prior to that at which any actual consideration of the relations of magnitude is introduced, and to which no skill in the management of such combinations can contribute, the most elevated mathematical genius may be but little appliable.

The nature of Laplace's reasoning in proof of the law, as he considers it, that force is proportional to velocity (p. 33), will appear from the following outline:

A body on the surface of the earth partakes in the velocity (v), and is impelled by the force (f) which belong to the earth's motion. Let us suppose the velocity to be some function of the force, or

$$v = f.\,\phi\,(f)$$

the object is to determine the form of this function. The force f may be considered as resolved into three a, b, c, parallel to three axes. Another force f' is now supposed impressed on the body, likewise resolved into a', b', c', parallel to the same axes.

He then obtains an expression for the resultant of these forces by means of developement, in which (since any forces impressed are very small compared with that of the earth's motion) we may neglect terms of two dimensions and upwards. Also, since the position of the axes is arbitrary, we may, for simplicity, suppose f'' impressed in the direction a so that b' and c' are $=0$.

On these considerations expressions obtained for the relative velocity of the body, or the difference between its motion and that of the earth, in the directions of three axes respectively, are reduced to the forms,

$$\left.\begin{array}{l} a'\left[\phi(f)+\dfrac{a^2}{f}\dfrac{d.\phi(f)}{d.f}\right] \\ \dfrac{ab}{f}.\ a'\dfrac{d.\phi(f)}{d.f} \\ \dfrac{ac}{f}.\ a'\dfrac{d.\phi(f)}{d.f} \end{array}\right\}$$

Here, *in general*, supposing b and c not $=0$ (i. e. that a, and therefore also the force f', does not coincide with the direction of the earth's motion) the body will have a velocity in a direction transverse to a.

But if we find *the differential coefficient* $\frac{d\,\phi(f)}{df}=0$ whatever be the value of f, the whole velocity will be reduced to $a'\ \phi(f)$: consequently $\phi(f)$ is constant, and *the velocity proportional to the force.*

In order to ascertain whether this condition is true in nature, or that a body so impelled has no motion in the directions b or c, the author refers to experiment: such as, that supposing a body on a perfectly level horizontal plane to receive an impulse in a given line, inclined to that of the earth's motion, the most exact observation shews that it has no deviation from that line, or no transverse motion. Or again, very accurate observations on the pendulum with its plane of vibration in that of the earth's motion, and perpendicular to it, must detect the smallest deviation, if any existed; but they do not. Hence he infers that the condition above stated of the differential coefficient being $=0$ is fulfilled in fact, and therefore the force proportional to the velocity is the law of nature.

On the nature of such results I have made some remarks in another place.

But further to illustrate this point, Laplace has devoted a chapter (liv. I. c. vi.) to the express consideration of the laws of the motion of a system of bodies *in all possible mathematical relations between the force and the velocity*. An infinite number of such relations he observes may be imagined *which imply no contradiction*. In the preceding chapters he had been investigating the motion and equilibrium of a system of bodies on the supposition of that simpler relation which he had proved to be the law of nature. He here proceeds to trace out all the *analogous* mathematical conclusions, introducing, instead of the simple relation of a constant proportion, the condition $F=\phi(v)$; ϕ being a function of any possible form. Or what amounts to the same thing, the expressions for the resolved velocities are here all *multiplied by the factor* $\frac{\phi(v)}{v}$, which in the former investigation was *unity*. This produces considerable analytical complexity: nevertheless he is able to deduce several very remarkable conclusions as to the existence of principles *analogous* to those of the virtual velocities, of the motion of the centre of gravity of a system, of the equal areas, and of least action, which had been deduced on the simpler hypothesis.

All this, it must be borne in mind, is purely fictitious mathematical speculation, turning upon the introduction of the particular expression above mentioned; which, as observed above, is equivalent simply to adopting a different definition of the term "force."

VII. (p. 34.)

The definition of the term *force*, to which I have here considered the so called first law of motion equivalent, is obviously the same (when we take it in the comprehensive sense, as well of the relation of actual quantities as of their limiting ratio) as that conveyed in the analytical expressions

$$v=\frac{ds}{dt} \qquad f=\frac{dv}{dt}$$

It is in this point of view that the subject is discussed by D'Alembert. [Elem. de Phil. Melanges, tom. iv. p. 197.] And also by Professor Playfair, who cautiously introduces the suggestion that it may be regarded rather as a definition than a theorem; and observes, " we have no distinct idea attached to the word *force* which we can compare with that conveyed by the formula." Hence he proceeds to propose the use of the word " force" as simply equivalent to the differential coefficient of velocity and time, and concludes, " *Force* in dynamics has in reality no other signification than this: the one expression may be every where substituted for the other, and thus an entire treatise on dynamics might be written in which the term *force* should not once occur." [Outlines, vol. i. p. 44.]

This passage has been commented upon by Mr. Whewell in a pamphlet entitled, " Thoughts on the Study of Mathematics, &c. Camb. 1835," where he observes, (immediately after giving the quotation, p. 27.) " If he had attempted to write such a work, he would have found the necessity of referring, at any rate, to the *conception* of force. How, without such a reference, would he have shewn that action and reaction are equal ?"

That in a work founded on this analytical definition it would be necessary to refer to the conception of force, appears to me to amount merely to this, that having reasoned upon force in its simple mathematical sense, it will become essential in a dynamical system to recur also to the other sense of the term, and to refer to the actual existence in nature of certain kinds of force, in the sense of agents or physical origins of motion. One of these is *pressure*, on the only intelligible sense of which I have before remarked. Action and reaction (so far as the terms have any meaning) appear to me referrible to the same considerations.

It is one of the principal objects in the pamphlet referred to, to maintain that some real conceptions of things are essential to mechanical reasoning besides definitions of terms, especially the conception of pressure. Thus (in p. 25.) the author refers to various elementary propositions, which, he says, "are all perfectly evident, as soon as we have formed the general conception of pressure, but without that act of thought they can have no evidence whatever given them by any form of words, or reference to other truths; by definitions or illustrations from other kind of inquiry." That it is quite essential to learn, from observation, the existence of certain modes of communicating motion, if our abstract reasoning is to apply to fact, is unquestionable. The nature of pressure (I have before had occasion to observe), though but vaguely understood in general, is certainly one of these species of physical forces; but the generality of its application seems to me overrated. The observations which follow on the definition of force, and the necessity of experimental conceptions before we can apply it, appears to me to accord with all I have advanced on this point; and, in fact, amounts simply to the recognition of the distinction I have laid down as to the two senses of the term *force*.

Again, the author observes (p. 27.), "It was nothing but the want of this distinct idea of pressure, or the loss of it after Archimedes, which made all the attempts of the ancients in mechanical reasoning entirely futile up to the time of Stevin. It was, for instance, impossible that men could reason on these subjects to any purpose, so long as their minds were in such a state that they could accept the fable of Remora or Echineis, a fish half a foot long, which, merely by sticking to the bottom of a ship, without any other hold, could prevent its going on, though impelled by the strength of four hundred rowers; and though the fish, when caught and laid on the deck, possessed no such power. Any one whose notion of force had acquired any degree of distinctness would at once see the absurdity of this, since the ship and its rowers must pull the fish as much as the fish the ship; a result which it would, however, be difficult to deduce from the definition of force $\frac{dv}{dt}$ without some accompanying idea."

That this fable is absurd will, without much difficulty, be allowed; but not, I conceive, owing to the law of action and reaction. It appears to me, that as we know not the limits of muscular power, there is no contradiction whatever in imagining a fish which should possess a power of muscular action, when in the water, equal to that of four hundred rowers; and a faculty of fixing itself to the keel, so as to exert this power in opposing the progress of the ship. That it is *absurd* to suppose this, arises merely from the *physiological* improbability of such a fish existing; I cannot perceive the smallest *mechanical* contradiction.

That the law of action and reaction cannot be deduced from the analytical expression of force, I readily admit; but from what has been said in the foregoing pages, it will hardly be necessary to dwell further on this point.

VIII. (p. 46.)

Dr. Brown argues strongly, on the same grounds as before, against the application of "sufficient reason" to demonstrate the composition of forces (On Cause and Effect, p. 288.); and more particularly maintains that we can in no way determine *a priori* what the mutual action of impinging bodies may be one upon another, and alleges, as a parallel case, that of chemical action. (p. 291.) He contends that the very term *composition of forces* is equivalent to begging the question, since we cannot be sure that after the moment of application there remain *two* forces to be compounded. (p. 298.)

Mr. Peacock, in the investigation of this subject introduced into his treatise on Algebra (p. 463. et seq.), with regard to the *resultant*, from the consideration that it is the natural effect of force to produce motion, infers that whatever number of forces act on a point, there can be *only one effect*, and therefore *one* equivalent or resulting force. This possibly may be thought a more directly satisfactory way of viewing the subject than that I have adopted.

His proof also subsequently turns upon the supposition that "the respective resultants of two sets of forces are proportional to their corresponding component forces." This he supports on the principle of sufficient reason. Here I confess, on the grounds before alleged, I feel a little doubt; but to those who find that principle satisfactory, this proof will certainly afford some simplification upon those of the French writers.

The investigations of Aristotle on the composition of motion are extremely curious, considering how little advanced his general notions of the nature of motion were. They occur in his Mechanica, §. 2. p. 28. ed. Monantholii, Lugd. MDC[a]. The object, in the first instance, is to shew why the extremity of the longer arm of a lever moves with greater velocity than that of the shorter.

In shewing how this takes place, he says, Αἴτιον δὲ τούτων ὅτι φέρεται δύο φόρας ἡ γράφουσα τὸν κύκλον. Ὅταν μὲν οὖν ἐν λόγῳ τινὶ φέρηται, ἐπ' εὐθείας ἀνάγκη φέρεσθαι τὸ φερόμενον, καὶ γίνεται διάμετρος αὐτὴ τοῦ σχήματος, ὃ ποιοῦσιν αἱ ἐν τούτῳ τῷ λόγῳ συντεθεῖσαι γραμμαί.

The phrase δύο φόρας had been nowhere before explained.

He continues this investigation by shewing the composition of rectilinear uniform motion, when the two component motions have a given ratio.

This is done by forming a rectangle, and then supposing one of the sides to be carried (φερέσθω), (parallel to itself) along the other; whilst the second side is in like manner carried (ὑποφερέσθω) along the first, to distances respectively in the given ratio.

He concludes (p. 29.), Φανερὸν οὖν ὅτι τὸ κατὰ τὴν διάμετρον φερόμενον ἐν δύο φοραῖς ἀνάγκη τὸν τῶν πλευρῶν φέρεσθαι λόγον. This being the case when the two motions are in a *given ratio.* He then comes to the case of circular motion, which is described in the following remarkable passage, immediately following the last quoted:

Εἰ γὰρ ἄλλον τινὰ, οὐκ οἰσθήσεται κατὰ τὴν διάμετρον. Ἐὰν δὲ ἐν μηδενὶ λόγῳ φέρηται δύο φορὰς κατὰ μηδένα χρόνον, ἀδύνατον εὐθεῖαν εἶναι τὴν φοράν. Ἔστω γὰρ εὐθεῖα. Τεθείσης οὖν ταύτης διαμέτρου, καὶ παραπληρωθεισῶν τῶν πλευρῶν, ἀνάγκη τὸν τῶν πλευρῶν λόγον φέρεσθαι τὸ φερόμενον· τοῦτο γὰρ δέδεικται πρότερον. Οὐκ ἄρα ποιήσει εὐθεῖαν τὸ ἐν μηδενὶ λόγῳ φερόμενον μηδένα χρόνον. Ἐὰρ γάρ τινα λόγον ἐνεχθῇ ἐν χρόνῳ τινὶ, τοῦτον ἀνάγκη τὸν χρόνον εὐθεῖαν εἶναι φορὰν διὰ τὰ προειρημένα. Ὥστε περιφερὲς γίνεται, δύο φερόμενον φορὰς ἐν μηδενὶ λόγῳ μηδένα χρόνον.

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[a] Vol. vi. p. 146, 147. Oxford edit. 1837.

His commentator considers this passage as obscure, and attempts to explain it by understanding the ἐν μηδενὶ λόγῳ to mean in no commensurable ratio.

The passage has been noticed by the author of the life of Galileo, (Library of Useful Knowledge, p. 81.) who in his translation renders μηδενὶ λόγῳ "an indefinitely small ratio," and μηδένα χρόνον "an indefinitely short time." Aristotle, however, is here more rigidly correct in his mode of expression than his modern commentator, if he had in mind the idea of a limit. On this supposition indeed he appears to have made a singular approach to the great discoveries of modern times. His assertion is equivalent to saying, that a body urged by two motions which are not comparable, except in their limiting ratio, will describe a curve. This certainly seems the most likely conjecture as to his meaning, though he does not further explain his mode of reasoning to this conclusion.

The student may find convenient the following references to some of the principal proofs of the parallelogram of forces, besides those already referred to.

The older proofs among modern writers, as those of Barrow (Lect. Geom.), and of Wallis (Mechanica, p. 654.), especially the latter, involve the assumption of forces acting in all lines parallel to a given line, under the phrase "secundum directionem."

Other proofs are those of Roberval (Mem. Paris. vi. 1.), Riccati (Comm. Bon. II. ii. 305.), Euler (Nov. Com. Pet.

xx. 188. 208.), D. Bernouilli (Comm. Pet. i.), D'Alembert (Opusc. i. Mém. 5.), Poisson (Méc. Anal. i. §. 13, 14.), Lloyd (Dublin Phil. Journ. i. 41.), Pratt (London and Edinburgh Journal of Science, No. 25.)

It will hardly be necessary formally to refer the student to the luminous comparative statement of the proofs of Laplace, Pontecoulant, and Poisson, given in the Appendix to Mr. Walker's Mechanics.

ON THE
COLLECTION OF BORACIC ACID
FROM THE
LAGONI OF TUSCANY.

BY

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ON THE
COLLECTION OF BORACIC ACID
FROM THE
LAGONI OF TUSCANY.

READ TO THE ASHMOLEAN SOCIETY, NOV. 20, 1837.

THE most remarkable natural phenomenon, of the many which are worthy of observation in the part of Tuscany called the Maremma[a], is the production of Boracic Acid from steam, which issues from the ground charged with that substance. This is the only spot in the world from whence this acid is obtained in a natural state, the salt called Borax, previously known in commerce, being brought from Thibet, where it is understood to be found in certain lakes, and as an incrustation on the ground, but certainly in a very impure state.

Borax appears to have been known to the Hebrews, Greeks, and other nations of antiquity, by names very nearly identical with that which it still bears. It is supposed to be the Chrysocolla of Pliny, being so called from its great use as a flux in soldering metals. Its present name is that given to it by an

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[a] The maritime district included between the Cæcina on the north, the sea on the west, the Roman road through Siena on the east, and extending on the south into the Roman states.

Arab chemist, Geber, in the 10th century. Though extensively employed in the arts, little or nothing was known either as to whence it was derived, or of its nature, before the beginning of the last century. It was known to be brought from some country in the East under the name of Tincal, mixed with a soapy substance, from which the Venetians had the secret of purifying it, a process which was afterwards carried on in Holland, by means of repeated washings, dissolutions, and crystallizations. At length (in 1702) some light was thrown on the true nature of this substance by an accidental decomposition of some, by the chemist Homberg, who thus for the first time obtained Boracic acid. It was only sixty years ago, in 1776, that Hoëffer first discovered the existence of Boracic acid in a natural state, in the waters of a lake near Monte Rotondo in Tuscany, which I visited in the spring of this year. By combining this natural acid with soda he succeeded in artificially composing Borax, which was thus proved, analytically and synthetically, to be a *sub-borate of soda*[b]. Lastly, in 1808 and 9, Gay Lussac, Thenard, and Sir Humphry Davy, proceeded a step further by decomposing the Boracic acid, and were thus enabled to study its base, called *Borium*[c].

Borax usually assumes, when pure, the form of a hexahedral prism compressed, and terminated by a trihedral pyramid. It will dissolve in twice its weight of water, if boiling, but requires much more water if cold. Its transparency is gelatinous and fracture glassy. It effloresces when exposed to the air, which arises from its containing half its weight of water of crystallization, and which also causes it to swell up when exposed to a strong heat, after which it melts into a glass, at first limpid, but afterwards becoming dull[d].

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[b] Fourcroy's Chemistry.

[c] Philosophical Transactions, 1809. and Thompson's Chemistry.

Borax is extensively used as a flux in causing other substances to melt, and is thus very useful in soldering metals, by causing those particles in immediate contact with it to melt at a less heat than is sufficient to fuse the adjacent parts, which are not exposed to its influence. This property makes it of great use in reducing substances before the blow-pipe, and also in causing the glaze on china and pottery to run, and in the manufacture of glass and enamel. Boracic acid might also be extensively employed in dyeing, were its price less high.

It was before observed, that Borax was for many ages brought from some country in the East, but the place where, and the circumstances under which it was produced, remained involved in mystery. Wishing to obtain some information on these points, Dr. Gilbert Blane, of the Royal Society, in the latter part of the last century, wrote to his brother in India, urging him to make inquiries which might throw some light on the subject; his reply to which is to be found in the Philosophical Transactions for 1787. Mr. Blane had not, however, an opportunity of visiting in person the localities where Borax is found; on the contrary, he obtained his account at the distance of thirty days' journey from the place, and he remarks himself, that the best information he could procure on the subject was "through some of the wild and unsettled mountaineers." The following are some particulars of his account. Having an opportunity of going from Lucknow to Betowle, a distance of 200 miles to the N. E. of the former place, he there met with some men from the Borax country. They informed him that it was found in the kingdom of Jumlate, in the mountains of Thibet, thirty days' journey north of Betowle. They said that in a valley, surrounded by snowy mountains, is a lake, six miles in circumference, the water of which is so hot that the hand cannot be held in it for any time. The ground around this lake is perfectly barren, not producing even a blade of grass: after a fall of rain saline concretions are formed on the ground. Upon the setting in of the snowy season, reservoirs are made in the ground on the banks of the lake, about six inches deep, and the snow having collected in these, hot water from the lake is thrown into them, which mixing with the melted snow is partly absorbed into the earth, and partly evaporated, when the Borax is left like a crust at the bottom. The process is only carried on in winter, snow being indispensable, nor can it be made again in the same place till snow has fallen and dissolved three or four times. The water of this lake is very foul, and appears to boil up in many places. The Borax is transported on the backs of goats, and being refined by boiling and crystallization, sells for fifteen rupees the maud at Lucknow. The country produces, besides, iron, copper and sulphur.

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[d] Thénard's Chemistry.

Such is the account given by some of the natives to Mr. Blane, which at any rate sufficiently explains the impurity of the Borax obtained from this quarter. The water of this foul boiling lake is left to evaporate without the application of any artificial heat, and without any previous deposition of its impurities, in excavations in the ground, instead of in vessels adapted for the purpose. It is not very easy to see what effect the snow can have, unless, by suddenly cooling the water, it may help to make it depose its salt in a solid form.

We have another account of the manner of collecting borax in the same volume of the Philosophical Transactions, read by Sir Joseph Banks, and written in Italian by the Father Prefect of the missions in Thibet, Joseph da Rovato. The Padre being at Patna, obtained an interview (by means of Bahadúr Sah, brother of the king of Nephāl) with a man from the borax country, who undertook to explain the matter partly in the Nephāl, and partly in the Hindostanee language. This man informed him that in the province of Marmè, twenty-eight days' journey north of Nepāl, and twenty-eight west of Lassa, the capital of Thibet, there is a valley eight miles across, containing two villages, *Scierugh* and *Kanglè*, the inhabitants of which live entirely by the sale of borax, the country being otherwise quite barren. In this valley is a lake, (Vasca di acqua,) and other smaller pools, at the bottom of which the borax forms naturally, and men entering the water feel it with their feet, forming a crust at the bottom, Four miles off, in the same valley, is a great abundance of salt. Ten days further north of Marmè is another valley called Tapré, where borax is also found. Blane writes the Indian name *Swagah*, and the Padre *Soaga*.

Since this paper was read, Professor Wilson has had the goodness to send me the following extracts on the subject, the first of which is from the travels of Messrs. Moorcroft and Trebeck, not yet published.

"One of the chief articles of transit in the trade of Ladakh is Borax, which is brought from Bhot, sometimes by the Bhotias themselves, but more commonly by the people of Lahoul, who convey it to Kulu and Chamba, where it is refined, and whence it is exported to the Panjab and Hindustan, for the use of braziers and silversmiths. It is bought in Bhot at about 120lbs. avoirdupoise for a rupee: by refining, it loses half its weight, but the remainder sells for five or six rupees. There is also a sort of smuggling-trade carried on in this article: the shepherds of these provinces receive a gratuity for pasturing the flocks of Kotock, Chamba, &c. on the borders of Lahoul, but they sometimes take half the flocks with them to Bhot for Borax, instead of leaving them to graze on the mountains, and are thus enabled to sell it at a lower price." Moorcroft and Trebeck's Travels, I. 355. See for an account of the Bhotia export of Tincal, by way of Kamaon, the Asiatic Researches, vol. XVII. p. 41.

Turner says, "Bootan, Nipal, Bengal, and Hindustan, are supplied with Tincal from Tibet. Its value is little more than that of the labour employed in digging it from the bed of a lake in which it is deposited, at the distance of about fifteen days' journey from Testoo Lomboo." Journey to Tibet, 371.

A further account is given by Mr. Saunders in the same volume, p. 406.

“ The lake from whence Tincal and rock salt are collected is about fifteen days' journey from Tissoolumboo, and to the northward of it. It is encompassed on all sides by rocky hills without any brooks or rivulets near at hand, but its waters are supplied by springs, which being saltish to the taste, are not used by the natives. The Tincal is deposited, or formed in the bed of the lake, and those who go to collect it dig it up in large masses, which they afterwards break into small pieces for the convenience of carriage, exposing it to the air to dry. Although Tincal has been collected from this lake for a great length of time, the quantity is not perceptibly diminished, and as the cavities made by digging it soon wear out or fill up, it is an opinion with the people that the formation of fresh tincal is going on. They have never yet met it on dry ground or high situations, but it is found in the shallowest depths, and the borders of the lake. From the deepest parts of the same lake they bring rock-salt, which is not found in the shallows. The waters of the lake rise and fall very little: it is at least twenty miles in circumference, and is frozen over for a great part of the year.”

The locality of Turner and Saunders cannot be the same as that of Moorcroft and Traill.

There is a brief statistical notice of Borax also in the Asiatic Researches, vol. XVIII. p. 234.

Borax, Tincal, (*Sansc.* Tankana,) Sohága (*Sansc.* Saubhágya) is very extensively used all over India for soldering, and as a flux for promoting the fusion of both silver and gold. It was invariably employed by the native melters in the mints, until the introduction of different vessels and more powerful furnaces obviated the necessity of its use." Professor Wilson adds; "No European, as appears from the above extracts, has visited the site of the lake, or lakes, where Borax is found, and our knowledge of it is yet vague and unsatisfactory. It is said to be found in Persia also, but I do not recollect any particular account of the situation in which it is met with."

The necessity however of importing Borax from these distant parts, and in the impure state in which it is there obtained, is obviated since the discovery of the existence of Boracic acid in Tuscany.

The district in which this acid is produced in Tuscany is included in a circle of about nine miles in diameter, situated between the towns of Volterra and Massa Marittima. Within this area are eight or nine different establishments for its collection, nearly all the property of a French gentleman of the name of Larderel. The first attempt to avail themselves of Hoëffer's discovery by the extraction of this acid from the waters of the lake near Monte Rotondo, was made by the former proprietor of the land in that part, about twenty years ago. With trifling capital and little knowledge he could only prove the possibility of turning to good account this steam, which had been for ages issuing from the ground in every direction, charged with wealth, but which hitherto had served only to create a superstitious fear in the minds of the ignorant inhabitants. Few were bold enough to approach the spot where the steam rushes up with a roaring noise from crevices in the ground, enveloping every thing in thick, stinking smoke; or if they did, it was only with a crucifix in their hand. Science and commercial enterprise were however destined to convert these objects of dread into a blessing to all the inhabitants of the deserted country in which they are situated, and to make them a mine of wealth to their proprietor.

Your approach to one of these interesting spots is announced by the appearance of a white vapour, filling generally a ravine on the slope of a hill, and concealing all other objects beneath it. It is not till you have approached to within a short distance, that you begin to distinguish houses and men, and soon you step out of your carriage in the midst of volcanoes, and find yourself enveloped in a steam of a peculiar odour, more acid than sulphureous. You see on every side steam bursting from fissures in the ground, which is hot to the feet, and pools of black, muddy water in a state of violent ebullition, whilst your ears are full of hissing and boiling sounds. Any body who has lately read Dante, cannot avoid the persuasion that he had these Lagoni in his mind when he described at the end of the 7th canto of the Inferno, the place of punishment of those whose crime had been the indulgence of angry passions.

Far murkier was the wave
Than sablest grain: and we in company
Of th' inky waters journeying by their side
Enter'd, tho' by a different track, beneath.

* * * *

The good instructor spake: " Now seest thou, son!
" The souls of those whom anger overcame!
" This too for certain know, that underneath
" The water dwells a multitude, whose sighs
" Into these bubbles make the surface heave,

" As thine eye tells thee wheresoe'er it turn.
" Fix'd in the slime, they say, 'Sad once were we,
" In the sweet air made gladsome by the sun
" Carrying a foul and lazy mist within:
" Now in these murky settlings are we sad.' "
Such dolorous strains they gurgle in their throats,
But word distinct can utter none.

Cary's translation of Dante.

The process employed for turning these issues of steam to account is as follows: Round the more considerable fissures a circular basin is dug, about four feet deep, and usually three or four yards across. These basins, which are called *lagoni*, being situated at different levels, the water of a rivulet is admitted into them, which mixing with the black mud at the bottom, is made to boil up violently by the issues of vapour within its circuit. The water is generally confined in each basin for twelve hours at a time, during which period it becomes saturated to a certain extent with acid from the steam which has passed through it. It is then drawn off from the higher basin to one beneath it, where it remains an equal length of time, till at length it reaches a building at the bottom of the hill, in which the process of evaporation is conducted. The immense profit derived from the lagoni is chiefly the consequence of a happy expedient for effecting the evaporation, which was first put in practice in 1827. Previous to this period, wood-fires were employed for this purpose, but in that year the heat of the natural steam was applied to the evaporation, which of course at once effected an immense saving of fuel, and at the same time enabled them to replace the iron, by leaden boilers. Having arrived at the building, as before mentioned, the water of the lagoni enters a cistern, where it is allowed to repose till it has deposited the mud which it held in suspension. This silt is said to be a useful application for cutaneous diseases in animals. Having cleared itself of impurities, the water is then drawn off from the cistern into flat leaden pans, under which some of the natural steam is conducted by brick drains, about two feet under ground, and by this heat is evaporated. This process requires about sixty hours, the water passing successively from the pans at the upper extremity, into others at the centre, and from thence into others at the lower extremity of the building, by means of leaden siphons. Having arrived at a proper state of concentration, it is then conducted into wooden tubs, in which it cools for about five days, during which the crystallization of the boracic acid takes place on the sides of the tubs, and on the stick in the centre. The water which remains is then pumped off, and passes again with fresh water through the evaporating pans. The acid having been removed from the tubs is then spread on the floor of a closed chamber to dry, by the heat of the steam made to circulate beneath it. The acid thus prepared is sent in casks to Leghorn, where, by the mixture of soda with it, it is made into borax, which is thus obtained much purer than it is found in nature. As to the annual produce of these works, I have no better authority than the statement of the chief director, who informed me that about 2,000,000lbs. of boracic acid is made by Mon. Larderel in the course of the year. This is exported to all parts of the world, and sells at Leghorn for *2l.* 8*s.* per cwt., which would make Mon. Larderel's gross income about 40,000*l.* per annum.

From this very few thousands are to be deducted for directors, workmen, buildings, carts and horses, and other necessary expenses, though all the workmen are well paid and lodged, and (by his wishes) all married men, whose families he educates. Besides carrying on the whole business on a liberal scale, so sudden a creation of wealth has had its natural effect in rendering him less careful of expense than is quite prudent; and having laid out a large sum in the purchase of a title of nobility, his equipages, houses, and fêtes to the grand duke, are in the most splendid style.

This property, rich as it already is, might, I conceive, be rendered more valuable, did not the monopoly which Mon. Larderel has secured, by purchasing all the country where this steam rises, render him indifferent to any trifling ameliorations in the very simple process which, as it is, produces such magnificent returns. The vapour which has passed through the water in the pools, and is then allowed to go off, has not evidently parted with all its acid, the remainder therefore is lost. It seems strange that some means cannot be found of obtaining the acid, which appears to be in a state of sublimation, at once from the vapour, for instance, by condensation. Many small issues of steam are at present neglected, the power of which might perhaps be very much increased by means of the boring machine. The laws which regulate the greater or less production of acid also deserve to be investigated. For instance, I was informed that dry weather, whether hot or cold, is favourable to its production. Hence the moist Scirocco, or S. W. wind is bad, and the Tramontana, or N. wind, the contrary.

Besides the steam containing boracic acid, it rises in some parts in a pure state, and might be employed for various domestic purposes, such as heating houses, cooking, washing, brewing, &c. It might also be used to ripen exotic fruits, being made to circulate in walls, and for rearing all the plants of the tropics under glass. Whether it could be employed as a mechanical power is more doubtful. The only purpose to which it is now applied, except that of evaporating the water impregnated with boracic acid, is the drying of chestnuts, which, in this country are ground, and used instead of wheat-flour. The fruit is exposed to the heat in metal or earthenware vessels, which are placed over the issues of steam. The most plentiful source of pure steam which I observed at Monte Rotondo, came out of a small cavern, beneath a ledge of limestone, on the surface of which, a portion of the steam being condensed, forms beneath a pool of naturally distilled water, much esteemed for its purity by the people of the country. On placing the ear within the mouth of the cavern, a roaring noise is heard, caused apparently by the escape of the steam through a narrow passage. This latter rock, unlike those exposed to the action of the acidulated steam, shews few signs of decomposition, and it is singular that a kind of Oscillatoria, which is also found in the Geysers of Iceland, as I am informed by Sir W. Hooker, is growing on the rock in the place where the hottest steam continually envelopes them, and thus from the peculiarity of their situation these plants seem to merit the attention of cryptogamists.

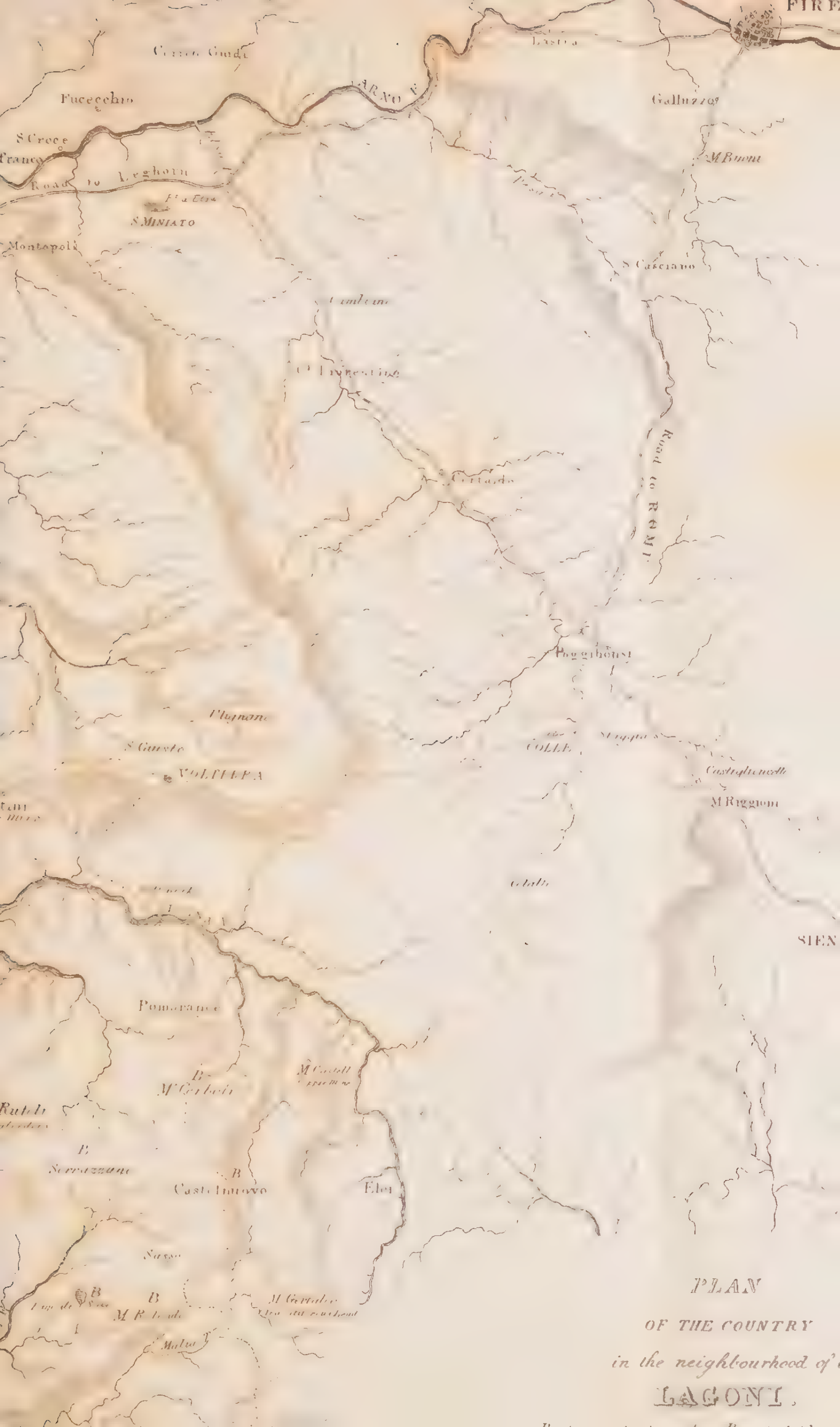
It now only remains to describe the geological formation through which these remarkable columns of steam make their passage. The principle rock which prevails at Monte Rotondo and Monte Cerboli, and indeed in a great part of Tuscany, (forming, as it were, the skeleton of the country, in the hollows of which are deposited the sub-Apennine marl and sands,) is a compact limestone, often becoming highly inclined and schistose, and called in Tuscany, Alberese. From the scarcity of organic remains in this rock, in which I never saw any, its position in the geological series has been doubted; but the general opinion seems to agree with that of Professor Savi, of Pisa, who places it at the bottom of the cretaceous group. However this be, it appears that the volcanic forces beneath this part of Italy, which have in other places heaved up serpentine through this rock, have here found a vent through it in the form of steam. This limestone is traversed at Monte Rotondo, by a hard siliceous, and quartzose rock, upon which the action of the acidulated steam is very remarkable, decomposing it so as to give it a porous, spungy appearance, and converting it into a white impalpable powder, excellently adapted for making china. A stone, if it happen to lie over a vent of this steam, usually has a thick layer of crystallized sulphur on its under side, and is rendered so fragile as to break away with the slightest effort. Small stones which have lain some time in the boiling lagoni, have an appearance of being bronzed on the surface. It would appear, from a consideration of the localities where this phenomenon is exhibited, that the steam finds less resistance in making its exit on the flanks, or towards the bottom of hills, than at their summits, to which it has a constant tendency to approach by eating into the hill, and rising continually nearer its culminating point. There may, perhaps, be less resistance in passing more or less horizontally between the strata, than in cutting them perpendicularly. Hence it would seem that the proprietor of the higher ground has it in his power to cut off the supply from his neighbour on a lower level, if, by the boring machine for instance, he can pierce to a stream of vapour, which would then rise perpendicularly in his property. Such was the idea of the state of things suggested to my mind by some experiments which I witnessed on a small scale. By the erosion of the rocks traversed by this steam, land-slips are caused, by which the passages through which the steam previously found an exit are stopped up, and it bursts forth at one or more points where there was no appearance of it before. Within the area where this phenomenon is exhibited, are also several hot springs, particularly at Bagni à Morbo, which are doubtless connected with the same subterranean causes. In the immediate neighbourhood of the lagoni are found salt, sulphur, alabaster, gypsum, serpentine, and chalcedony, of which last the beautiful works in *pietra dura* are executed at Florence.

Near Monte Rotondo, but in a valley someway beneath Mon. Larderel's establishment, is the lake of Sasso, through the waters of which a quantity of steam rises, and from whence boracic acid was first obtained. This however is all now wasted, a dispute having been raised as to the person in whom the property, now become so valuable, vests. I mention this lake, not only on account of its interest in the history of this remarkable mineral, but also of a curious tradition as to its origin, which was related to me by a countryman, who was my guide in this neighbourhood. I must first beg the Society not to be shocked by the introduction of our Saviour's name, as it shews the degree of historical religious instruction which these people receive.

The following is an extract from my journal. "From the heights of Serrezzano you see to the right steam ascending from the lagoni of Sasso and Monte Rotondo. The sight of this eternal smoke rising from different parts of the plain below me, naturally reminded me of the striking account of the destruction of Sodom and Gomorrah; whilst thinking of which, my guide commenced the following narrative: 'In the place of that boiling lake from which rises the smoke which the signore sees down there, was once a *podere*, (i. e. a small peasant's farm.) It was the 5th of August, the festival of la Madonna della Neve, when our Saviour passed that way, and seeing the master of the house busy at work, he asked him if he were not going to mass? No, said the man, what *festa* is it to-day? It's the festival of our Lady of the Snow, answered our Lord.——Well, said the man, I've other business to-day, I can't go to mass. So when our Saviour found he could not persuade the man or his wife to go, he said, If *you* are too busy, at least let those two little boys go with me, they have nothing to keep them away. This was agreed to; so, taking the children by the hand, they departed together. They had not got far up the hill-side, when our Saviour turned round, and bid them look over the plain towards where their home had been, but it was all gone, and nothing was to be seen of all their vineyards, olive-fields, or their house, but in their place was that boiling, smoking lake.'"

The above are a few of the more obvious remarks suggested by the sight of these remarkable localities, and which I have been induced to offer to the Society, partly because I am not aware of any description of them having been yet given in English, and partly in the hope of inducing some member, more competent than myself, to do justice to the subject, to step a few miles out of the high road to Rome, and devote a few days to their study. For this purpose a knowledge of chymistry would be perhaps as necessary as of geology, in order to observe the various sublimations and depositions which are continually going on in this natural laboratory. Indeed, I was informed that the French chymist Payen had been employed, at the express invitation of their proprietor, in collecting all the products, solid or gaseous, which he could discover in the neighbourhood of these lagoni, which he was to examine on his return to Paris; but whether his report upon them has been made public, I am not aware. At all events to any traveller, whether scientific or not, I can safely recommend a trip from Florence, to see the very interesting Etruscan museum at Volterra, whence it is a short ride to a peculiar copper-mine at Monte Catini, and another short ride to Pomerance, the residence of Mon. Larderel, and thence to Monte Cerboli, where is the largest establishment for collecting boracic acid, and the road is practicable for a carriage.

If inclined to extend his journey in the Maremma he will find it a most interesting country, containing the remains of Etruscan cities and tombs, mines of copper, probably worked by that nation and in the middle ages; others of antimony, iron, lead, alumn, and plants and insects, which are also natives of Africa. Some very interesting works are also in progress on a great scale, for rendering the country more healthy, by filling up the pestilential marshes by means of rivers turned into them, and there allowed to deposit the abundance of silt carried down by them. Near the Pope's frontier, and from thence to Rome, are a succession of extinct volcanos which are also well deserving of investigation.



PLAN

OF THE COUNTRY

in the neighbourhood of

LAGONI.